THURSDAY, MARCH 24, 1892.

THE HISTORY OF DETERMINANTS.

The Theory of Determinants in the Historical Order of its Development. Part I. Determinants in General. By Thomas Muir, M.A., LL.D., F.R.S.E. (London: Macmillan and Co., 1890.)

THE theory of determinants is in that borderland which separates the "pass" from the "honour" student of pure mathematics. In elementary text-books the subject is rarely more than introduced for the purpose of representing some result of geometry or analysis in a convenient, beautiful, and suggestive form. The essential properties of a determinant are not set forth, but the student is perhaps referred for further information to one or other of the two excellent treatises which are already at our disposal in the English language, viz. those of Mr. Muir and of Mr. R. F. Scott. The value of the idea thus given to a student of the shape and convenient use of a great analytical implement is beyond all question. His imagination and curiosity are alike excited, and the "trick" possibly prevents his passage through life under the delusion that all mathematics are comprised within the covers of the school-books. The honour student, as a matter of course, reads some work on the subject, and is as surely enchanted. He cannot fail to recognize the power and beauty of the notation. He observes that the object of his study is constructive in its nature. He becomes convinced that pure mathematics is one of the fine arts, and just as a beautiful picture gives pleasure to one who understands painting, just as a fine piece of sculpture delights one who understands modelling, so he sees unfolded to his intellectual eye an exquisite example of constructive art, which his previous mathematical reading has fitted him to understand and appreciate, and to regard as a beautiful object of contemplation. The theory of determinants is one of the most artistic subdivisions of mathematical science, and accordingly has never wanted enthusiastic admirers. It is most gratifying to find such an authority as Mr. Muir devoting his leisure to its historical development. Any mathematician taking up this volume would anticipate a treat, and he would not be disappointed. In this first instalment the reader is taken from Leibnitz (1693) to Cauchy (1841). Mr. Muir assigns a chief place of honour to Vandermonde (1771), who, in his "Mémoire sur l'Élimination" (Hist. de l'Acad. Roy. des Sciences), denoted a function formed from the coefficients of a set of linear equations by a symbolism which is at once recognized as a condensed form of the determinant matrix of the present day. He was the first to give a connected exposition of the theory, and to give the true fundamental properties of the new functions. His notation, moreover, was exceedingly good, and much superior to that adopted by some subsequent writers who overlooked or neglected his important work.

Vandermonde has also recently received justice, long denied him, in other branches of analysis, and there is now no doubt that the value and originality of his work entitle him to a place in the ranks of the mathematical pioneers of his time. Up to the close of the eighteenth century the most noteworthy additions were made by Laplace, Lagrange, and Bezont. We find that Lagrange knew that the discriminant of a binary quantic of the second order is an invariant of the linear transformation. He did not, however, express either the discriminant or the determinant of transformation in a determinant form. The author critically examines the claim of Laplace to be the discoverer of the expansion theorem. He finds that although the case in which as many as possible of the factors of the terms of the expansion are of the second degree had already been given by Vandermonde, and Laplace himself did not give a statement of the rule suited for general application, the claim can in the main be upheld. Hindenburg (1784) and Rothe (1800) took up the subject in Germany, and between them constructed an elementary theory of permutations. Rothe, it is interesting to observe, employed a graphical method which will remind the reader of Prof. Sylvester's modern constructive theory of partitions. Gauss (1801) followed, and then we come to the important papers of Binet (1811) and Cauchy (1812). These memoirs establish the multiplication theorem in its full generalization. The method adopted by Binet may be described as that of symmetric functions, which he uses freely. He employs identities of the type

$$\Sigma ab'c'' = \Sigma a\Sigma b\Sigma c + 2\Sigma abc - \Sigma a\Sigma bc - \Sigma b\Sigma ca - \Sigma c\Sigma ab,$$

having reference to several systems of quantities. He was not, however, sufficiently acquainted with the theory of such functions; and was unable to supply rigid proofs of the theorems in determinants which, from his point of view, rested upon these identities. Nowadays, the identity in question will be recognized as the expression of an "elementary" symmetric function (single unitary, and having three parts in its partition) by means of symmetric functions each of which is expressible symbolically by a partition composed of one tripartite part. The law of the coefficients, undivulged by Binet, is now perfectly well known. It is, in fact, an easy generalization of the law by which, in the case of a single system of quantities, the elementary symmetric functions are expressed as functions of the sums of powers of the quantities. Cauchy at the same date (1812) introduced the idea of "fonctions symétriques alternées," which led him to a new symbolic definition of a determinant and to many valuable results. Mr. Muir devotes several pages to an examination of Cauchy's title to share with Binet the credit of the generalized multiplication theorem. He gives his decision against Cauchy, and probably the reader who closely follows the argument will find himself in accord with the historian. Notwithstanding this conclusion, Cauchy's memoir is excellent in quality and abundant in quantity; he "opened up a whole avenue of fresh investigation, and one cannot but assign to him the place of highest honour among all the workers from 1693 to 1812."

A retrospect is given of the period 1693–1812 accompanied by an interesting tabular record. As a means of reference the work appears to be absolutely perfect. Each new result as it appears is marked in Roman figures; and if the same result be obtained differently, or be generalized by a subsequent investigator, the same Roman figure is employed, followed by an Arabic numeral. It is found that to this point nearly every important advance is due to French mathematicians.

During the second period (1813-1841) the chief names are Cauchy, Schweins, Jacobi, and Sylvester; to these may be added Desnanot and Scherk, to whom fresh departures, of less extent, are due. Schweins himself may be said to have been discovered by Mr. Muir. This author (1825) deals with the subject under the title "Theorie der Producte mit Versetzungen." He made a notable generalization described as the transformation of an aggregate of products of pairs of determinants into another aggregate of similar kind. He further discussed special forms, and, it is clear, possessed a firm grasp of his subject.

The work of Jacobi and Sylvester, and the further work of Cauchy will be more or less familiar to mathematicians. Germany has passed to the first place; and the occurrence of Sylvester's name in the history marks a revival of learning in pure mathematics in England.

The volume is remarkable for the study it presents in nomenclature and notation. There is an extraordinary variety in the symbolism. It is easy to observe the distinctive characters of French and German notation that are so marked at the present day. It is well known that much lies in an appropriate notation. Every young mathematician with a predilection for original work should read this book, to note the power of suitable symbols, to grasp the reason of their power, and, above all, to see what to avoid.

The book also points a moral which is not far to seek. It would be easy to pick out many such phrases as, "was acquainted with the writings of very few of his predecessors"; "was unaware apparently of the existence of a theory of determinants"; "hasty, if not contemptuous, disregard of historical research." To this tendency to work on without proper attention to previous work is doubtless due in some measure the unfortunate multiplication of names and symbolisms which is so perplexing and irritating to a reader. This failure to collaborate with others can only retard the progress of the science. It is perfectly true that great original thinkers, like Gauss, Cauchy, Jacobi, and Sylvester, may take liberties of this kind; and the fact of their doing so may even be beneficial to the subject, as resulting in memoirs of more unfettered originality. But this is not so in the case of lesser men. In taking leave of this fascinating history one can look forward to Part II. with sincere pleasure, which is not diminished by the knowledge that the later developments have been largely due to our countrymen. We have yet to see Sylvester's most powerful investigations, and all Cayley's researches; and, finally, the successive steps by which the lofty heights of the theory of matrices and the theory of multiple algebra (involving the generalization of quaternions) have been attained.

P. A. M.

THE EVOLUTION OF MAN.

Anthropogenie, oder Entwickelungsgeschichte des Menschen. By Prof. Ernst Haeckel. Fourth Edition, revised and enlarged. Pp. i.-xxvi. and 1-906. (Leipzig: W. Engelmann, 1891.)

THE importance of the subject-matter of the book, the length of time that has elapsed since the appearance of the former editions, and the prominent position held by the author, seem to call for more detailed notice of this work than is usually accorded to a new edition.

The "Anthropogenie," which was first published in 1874, is the third of the series of books in which Prof. Haeckel has attempted to determine the laws governing the form, structure, and mutual relations of living things, and to establish the general principles of biological science. The first of these, under the title "Generelle Morphologie," appeared in 1866, almost simultaneously with the completion of Herbert Spencer's "Principles of Biology." It is a comprehensive and ambitious work, which, in its author's words,

> "constituted the first attempt to apply the general doctrine of development to the whole range of organic morphology, . . . and to introduce the Darwinian theory of descent into the systematic classification of animals and plants, and to found a 'natural system' on the basis of genealogy: that is, to construct hypothetical pedigrees for the various species of organisms."

It contains also the first systematic attempt to deal in detail with the ancestry of man, as regards the groups of animals lower than mammals. This is, perhaps, the most solid piece of work Prof. Haeckel has done; it contains much matter of great value, and discussions and speculations of extreme ingenuity and interest. Later discoveries have rendered much of it obsolete, but it still remains the most important work of its kind; and but for its somewhat cumbersome terminology, would be widely read even now. The "Natural History of Creation," first published in 1868, goes over a good deal of the same ground as the earlier work, but is written in a much more popular style, and aims at presenting in a form suited to the general reader the main arguments on which the Darwinian theory is based, together with a detailed application of the theory to the principal groups of animals, and an attempt to determine their mutual relations and lines of descent. The "Anthropogenie," the book now before us, is a more elaborate application of the same principles to the special problem of the evolution of man.

In the new edition the general arrangement remains much the same as before; but, in order to include the results of more recent investigations, a great part of the book has been re-written, and new chapters have been added on subjects that have attracted especial attention of late years, such as the Gastræa theory, the Cœlom theory, and the nature and origin of segmentation. A large number of new figures have been inserted, and the genealogical tables, for which Prof. Haeckel has a special fondness, have been greatly increased in number, and in elaboration of detail.

The book, which is adapted rather for the general reader than the scientific student, is written in an attractive and popular style, and presents the main facts of vertebrate embryology in an intelligible and well illustrated form. As might be expected from his former writings, the main feature of Prof. Haeckel's work is a detailed exposition and vigorous defence of what he has named "the fundamental law of biogenesis," better known in this country as the recapitulation theory, according to which the actual or ontogenetic development of a animal is a repetition of the ancestral or phylogenetic

development of the species; or, to put it more simply, animals in their development climb up their own genealogical trees. This is now generally accepted by embryologists, but it has not always been so, and Haeckel reminds us, with justice, that he was one of the first to realize and teach the doctrine.

Prof. Haeckel has much to say on other points of theoretical interest. He protests strongly against Weismann's views with regard to heredity, pointing out that the very existence of germ-plasma is as yet a mere assumption, and maintaining that acquired characters may be and are actually transmitted. He objects equally strongly to the views as to the widespread occurrence of degeneration, which were first put forward by Dr. Dohrn; and on the much-debated question of the origin of Vertebrates he sides with those who fully accept the evidence afforded by the anatomy and development of Amphioxus; he admits the Vertebrate affinities of Balanoglossus, and looks for the ancestors of Vertebrates among the unsegmented Turbellarian worms. As a controversialist Prof. Haeckel is impressive rather than convincing. He hits hard and with effect, but prefers to counter rather than to parry the blows of his opponent. It is impossible to pass over without protest the terms in which he writes—it must be admitted under provocation—of the opinions and work of other investigators.

Prof. Haeckel's fondness for genealogical trees, and his facility in constructing them, are well known and have been much criticized, perhaps a little unfairly. Acceptance of the doctrine of evolution involves the recognition of a blood-relationship, near or remote, between any one animal and any other; and the only true classification is one which places this fact in the forefront, and adopts it as the basis on which the scheme is to rest. Genealogical tables undoubtedly stimulate inquiry, and so long as it is realized that they are necessarily in great part tentative or provisional, they probably will do more good than harm. It would be easy to take exception to many points in Prof. Haeckel's numerous and elaborate pedigrees, but it will be generally admitted that they are instructive, and often extremely suggestive, even though the conclusions may not meet with general acceptance.

The least satisfactory part of the book is that which deals with human embryology. No attempt whatever is made to explain the earlier stages of development; the special difficulties of the problem are absolutely ignored; the human gastrula is spoken of in a confident way, as though such a stage really existed; and the accounts of the development of the several organs and systems are too often taken from other animals. A student who relied on Prof. Haeckel's descriptions would obtain an entirely erroneous idea of the actual course of development of the human embryo.

Owing to the difficulty of obtaining material in proper condition for microscopical examination, our acquaintance with human embryology long remained imperfect; and the descriptions in text-books were largely based on our knowledge of other Vertebrates, and illustrated by figures from embryos of dogs, pigs, rabbits, or even chicken and dogfish. The time for all this is now past. During the last ten years our knowledge has advanced wonderfully; and although the earliest stages are still unknown, it is not too much to say that our knowledge of the development of the human embryo, from a stage corresponding to a chick embryo at the commencement of the second day onwards, is as satisfactory, as complete, and as well illustrated as that of any other mammal.

For this great advance we are indebted almost entirely to the labours of German embryologists, and notably to the splendid work of Prof. His. Prof. Haeckel has in his volume many hard things to say of Prof. His, but is indebted to him for the only really good figures of human embryos which he gives, and would have materially improved his book had he studied more carefully the admirable descriptions of the Leipzig Professor. It is a matter for great regret that a book of 900 pages, having for its title, "Anthropogenie, oder Entwickelungsgeschichte des Menschen," should be allowed to appear in which the account of the actual development of the human embryo is so inadequate or even erroneous.

A. M. M.

OUR BOOK SHELF.

Philosophical Notes on Botanical Subjects. By E. Bonavia, M.D. With 160 Illustrations. 368 pages. (London: Eyre and Spottiswoode, 1892.)

DR. BONAVIA'S philosophy is concerned with the evolution of vegetable organisms, and the gist of it is that all land-plants have descended from sea-weeds. He sees modifications and traces developments not obvious to ordinary observers, and he is prepared for derisive criticism. To quote from his preface:—"The fact is that, in this stage of existence, certain thoughts are often a great worry. One often cannot get rid of them. They turn up by day, they turn up by night, they turn up in the morning, and they haunt one at all times, and the only remedy for mitigating this worry of civilization is to commit them to paper. This done, there are several ways of disposing of your written thoughts. You can burn the papers they are written upon or otherwise destroy them, or you can leave them in a drawer as a legacy to your heirs! If by neither of these processes can you entirely give yourself repose, then the most effective way of ridding yourself of the worry of such thoughts is to have them published (if any publisher will perform this kind office), and to see them adversely criticized if anyone will even take so much trouble."

As the book before us testifies, Dr. Bonavia's worry reached the acute stage, and he is so far relieved as to have found a publisher; but we do not propose to gratify him by adverse criticism. We prefer giving one short extract from his sixteen "general conclusions":—

"Fifteenth:—The fig is obviously a further development of a conceptacle of a *Fucus* or other sea-weed. And there is every reason to believe that the oil-glands of the bark, leaves, and peel of the *Citrus*, and similar glands in other plants, are mere remnants of sea-weed conceptacles—that is, persistent features turned to other uses."

W. B. H.

The Zoological Record for 1890. Edited by Frank E. Beddard. (London: Gurney and Jackson, 1892.)

THIS is the twenty-seventh volume of the Record of Zoological Literature, and it has been prepared on the same plan as the volume published last year. First of all there is a list of works on general subjects, by J. A. Thomson. Then come the titles of writings on the following:—Mammalia, by R. Lydekker; Aves, by R. Bowdler Sharpe; Reptilia, Batrachia, and Pisces, by G. A. Boulenger; Tunicata, by W. A. Herdman; Mollusca, Brachiopoda, and Polyzoa, by P. C. Mitchell;

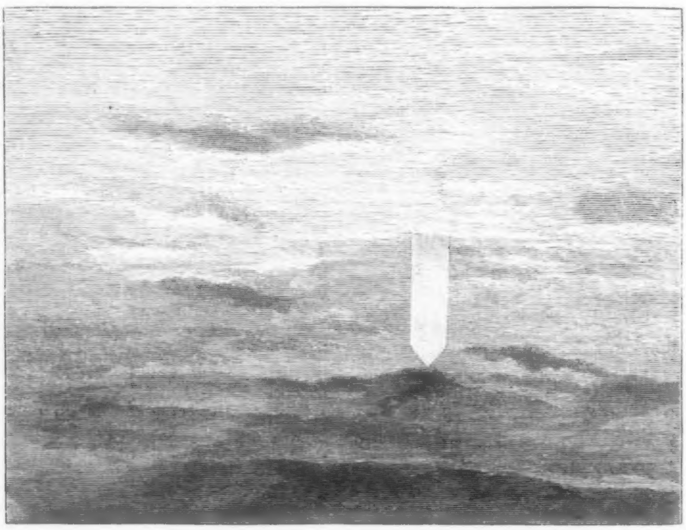
Crustacea, by C. Warburton; Arachnida and Myriopoda, by R. Innes Pocock; Insecta, by D. Sharp; Echinodermata, by E. A. Minchin; Vermes, by P. C. Mitchell; Cœlenterata, by S. J. Hickson; Spongiæ, by E. A. Minchin; Protozoa, by C. Warburton. The utmost pains have been taken to make the lists complete and accurate, and to students of zoology they are practically indispensable. In the introduction to Mammalia, Mr. Lydekker notes that the number of new recent species is extraordinarily large. He adds, however, that this is "due to the elevation to specific rank of a host of North American forms which would be regarded by most zoologists as varieties." No fewer than forty of the "new species" belong to this category.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Sun Pillar.

A REMARKABLY well-defined instance of this phenomenon was seen by me at this place (460 feet above mean sea-level) this afternoon. At 5.32 p.m. the sun was sinking behind a thick layer of stratus cloud. There was a bank of dust haze, so defined as almost to resemble cirrus, which apparently formed a background to the clouds. When the phenomenon was first noticed, about three-quarters of the sun's disk was below the edge of the cloud bank; and from the centre of that portion of the disk visible there rose a tall column of brilliant light, extending upwards to about 5°, of the same width as the apparent diameter of the sun, and narrowing almost to a point as it touched the sun's rim. This convergence became more marked as the rest of the disk disappeared, until at the point at which the latter was finally lost to sight the apex appeared to rest on the edge of the cloud bank. The cone-shaped part at the base of the pillar was the most luminous portion, and glowed with a brilliant orange-red tint, which gradually merged into the yellow-white of the upper part of the column. The effect lasted for some minutes after the sun's disappearance, but the pillar lost its conical base and became less defined, while the clouds receding gave the appearance of the base of the pillar having risen in the sky.



Sun pillar observed at Lutterworth.

I had noticed, a quarter of an hour previously, that the rays of the sun, when behind a cloud, stood out in an unusually solid and clearly defined manner. There was a good deal of anti-cyclonic stratus (about 5000 feet) at the time, and the upper part of the atmosphere was more hazy than is usual with a north-east wind at this period of the year. At the earth's surface the wind had dropped to an almost perfect calm.

Lutterworth, March 5. ANNIE LEY.

New Comet.

THE comet discovered here on the evening of Friday, March 18, is extremely small, though not very faint, and it has a decided central condensation or nucleus. Its position at about 8h. 30m., March 18, was roughly determined as R.A. 341°, Decl. N. 59°. The comet was therefore situated in Cepheus, and about 3° east-north-east of the star Delta in that constellation.

On March 19, at 8h., I reobserved the comet, and found its rate and direction of motion to be 47′ of arc east, and 12′ north. It will therefore shortly traverse Cassiopeia.

The comet was discovered with a 10-inch reflecting telescope, with eye-piece magnifying 40 times, and having a field of 65′ of arc. W. F. DENNING.

Bristol, March 21.

[This is stated to be Winnecke's comet.—ED.]

First Visible Colour of Incandescent Iron.

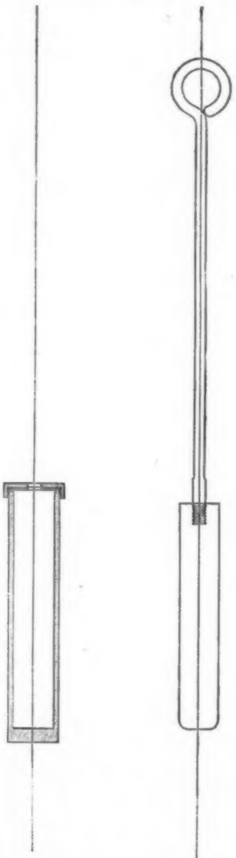
DURING the discussion which followed the reading of the paper on "Colour Photometry" by Captain Abney and General Festing at the Royal Society on January 28, some interesting remarks were made by Lord Rayleigh as to the colour exhibited by heated iron when raised to such a temperature as only to be just visible in a dark room.

Lord Rayleigh stated that Weber, who, so far as I know, first drew attention to this subject, described the first visible light as a greenish-grey. Lord Rayleigh himself repeated the experiment by making a piece of thin iron part of the wall of a very dark room, and heating the iron gradually by a Bunsen burner upon the other side. Lord Rayleigh could not satisfy himself as to the greenish tint, but was satisfied that no redness was apparent.

It struck me that a very convenient method of trying this experiment would be to introduce a round bar of heated iron into a thin sleeve, as shown in the annexed sketch, the sleeve being closed with a cover lined with asbestos. In this way the heat would slowly penetrate the sleeve, and the observers could note the first appearance of visibility and the changes of colour that followed.

I accordingly had two sleeves prepared, one of turned and polished iron, the other left with a thick coating of oxide. Two sets of experiments, in each of which six observers took part, were made. In each set of experiments three observations were made with the polished, and three with the oxidized sleeve. In each case the observers were in the dark room for some minutes before the experiments began.

In the first set of experiments the observers gave their opinion, at the conclusion of the experiments, as a body, that the first appearance of colour was a greyish-white; as the sleeve became hotter the colour was yellow, and gradually changed into orange. There was little or no difference between the observers as to the instant of visibility; it was generally over a minute before the sleeve became visible, the light generally showing first on the generating line of the cylinder between the eye and the axis.



There was no difference in colour between the bright and the oxidized sleeves.

In the second set of experiments, the observers had no communication with one another, had no idea what colour they were expected to see, and their impressions were written down separately and independently. Their impressions were as follow, the observers being designated by α, β, &c.:—

(α) First colour visible, grey white, second colour white with a little mauve, third pale rose, fourth orange. The above was the first experiment (polished metal). The other experiments showed same colour, but no mauve seen. In the last experiment (a very low heat) the colour never passed beyond a pale yellow.

(β) For all experiments, first grey white, second yellow, third orange. Last experiment, no orange.

(γ) For all experiments except last, first white, second yellow, third orange.

(δ) For all experiments except last, first grey white, gradually becoming warmer till it reached orange.

(ε) First white like phosphorus in the dark, gradually getting to rose, and winding up with a reddish-orange not reached in the last experiment.

(ζ) First white with a dark shade, second yellow, third orange; no difference in any of the experiments except the last, where the temperature was lower, and the orange was not reached.

I may add that the temperature of the heating bar was a little reduced each experiment, the colours changed very slowly, and gave ample time for observation. A. NOBLE.

Poincaré's "Thermodynamics."

M. TAIT ne répond pas à mon objection sous prétexte qu'elle est sans importance. Je maintiens que nous n'avons aucun moyen non seulement d'assigner l'origine des forces électromotrices Thomson, mais encore d'en constater l'existence. Si M. Tait veut répondre, et s'il connait ce moyen, qu'il l'indique. Dans le cas contraire, s'il n'est pas en mesure de soutenir une quelconque de ses critiques, et s'il préfère un autre terrain de discussion, je suis prêt à l'y suivre.

Seulement je serai forcé d'être un peu plus long, car il me faudra passer en revue les trois reproches de M. Tait.

(1) La forme de mon ouvrage est trop mathématique.

C'est là une appréciation personnelle dont il n'y a pas à disputer. Je veux bien d'ailleurs d'une polémique sur une question de doctrine, mais non d'un procès de tendance où je jouerais le rôle d'accusé.

Toutefois il est certain que je consacre relativement peu de place à la description des expériences, et on aurait le droit de s'en étonner si je n'en donnais l'explication. Mon livre est la reproduction textuelle de mon cours; or mes auditeurs avaient tous suivi déjà un cours de physique expérimentale, où ces expériences leur étaient décrites en détail. Je n'avais donc qu'à leur en rappeler brièvement les résultats.

(2) J'ai mal parlé de la définition de la température absolue.

Autant que je puis comprendre, M. Tait ne trouve pas ma définition mauvaise, et n'en propose pas une autre; mais, dit-il, j'aurais dû parler des expériences de Joule et Thomson, qui permettent de mesurer la température absolue.

Or j'ai décrit ces expériences à la page 164, et j'ai montré à la page 169 comment elles permettent de déterminer la température absolue.

(3) J'ai laissé complètement de côté une explication mécanique du principe de Clausius que M. Tait appelle "the true (*i.e.* the statistical) basis of the second Law of Thermodynamics."

Je n'ai pas parlé de cette explication, qui me paraît d'ailleurs assez peu satisfaisante, parce que je désirais rester complètement en dehors de toutes les hypothèses moléculaires quelque ingénieuses qu'elles puissent être; et en particulier j'ai passé sous silence la théorie cinétique des gaz. H. POINCARÉ.

Ornithology of the Sandwich Islands.

HAVING just returned from an exploring expedition into the interior of Australia, on my way home I lingered in this group of islands, and was sorry to find that some species which have been obtained here are now no longer to be found.

My attention has been called to an interesting paper by Prof. Newton in your last issue (p. 465), on this subject, which seems to imply that the ornithological collection made by Sir Joseph Banks during his voyage in the *Endeavour* with Captain Cook no longer exists, which I beg to be allowed to make a few remarks upon. After the return of Sir Joseph Banks he had several cases of birds carefully mounted and arranged according to the localities in which they were collected. In one group of land birds from Owhyhee, another case contained a number of specimens from Botany Bay, conspicuous in the centre of which was a very fine specimen of the Black Swan, which was shot by Captain Cook himself.

These cases were in the custody of the Linnean Society of London until 1863, when they formed part of their Natural History sale.

These cases have been carefully preserved, and are now in the museum of my ancestor, Mr. John Calvert, together with a number of cases of birds which formed part of Sir Ashton Lever's collection, amongst which are a few from the Pacific Islands. These last cases were purchased from the executors of the late Mr. M. Armfield, of Catherine Street, Macclesfield,

about twenty-five years ago, being a very fine collection of many thousand ornithological specimens, with a quantity of interesting correspondence with Mr. J. Thompson, of Knowsley Aviary, Mr. Reid, of Doncaster, R. Dunn, of Hull, and many other naturalists of that period; these explain upon what terms he obtained the egg and a very fine specimen of the Great Auk. ALBERT F. CALVERT.

63, Patshull Road, Kentish Town, March 19.

Superheated Steam.

LORD RAYLEIGH (p. 438) rebuts my objection to the statement regarding the efficiency of a vapour-engine in which pure water is replaced by a saline solution, pointing out "that Maxwell's exposition of Carnot's engine applies *without the change of a single word*, whether the substance *in the cylinder* be water, mercury, or an aqueous solution of chloride of calcium." The latter italics are mine. In the statement objected to by me the aqueous solution of chloride of calcium was *in the boiler*, and what was *in the cylinder* was *superheated steam*, which is not included in the above list, so that the application of Maxwell's exposition is somewhat difficult. The greater part of the fresh water supplied to passengers in steamships is now produced by condensing the *superheated vapour of a saline solution*, and the culinary experience is that the substance which was in solution has all been left in the boiler. My contention, therefore, still stands—the saline mixture is not the working substance, and Carnot's law refers to the working substance only, and not to anything left in the boiler.

"In each case there is a definite relation between pressure and temperature." This is evidently merely a slip of the pen, the writer having for the moment forgotten that he was dealing with *superheated* steam, for which there is not a definite relation between pressure and temperature. The condition of superheated steam is completely defined when *both* pressure and temperature are given; but pressure is here a function of temperature-and-something-else, and temperature is here a function of pressure-and-something-else. That something-else may be volume or it may be energy, or, preferably, it may be entropy, but it must be something which cannot be predicated from pressure alone or from temperature alone.

"(So far as the substance is concerned), all that is necessary for the reversible operation of the engine is that the various parts of the working substance should be in equilibrium with one another throughout." No; for, in addition, it is necessary that the working substance should have only one pressure consistent with any given temperature. For this reason, superheated steam, however it may have been produced, can never be the working substance in a Carnot's engine. In the reversed cycle, when the steam is raised from a saline solution, from the beginning of the higher isothermal, the pressure would go on increasing until it became that due to saturated steam at the temperature of the superheat. This might be double the maximum pressure in the original cycle.

"The various parts of the working substance should be in equilibrium with one another throughout." The writer seems to say that the steam of a saline solution is a stable saturated vapour. It is H_2O at a given pressure and temperature, and the condition of the substance is by this definition completely determined, and there is no alternative; but it is not stable. Say that the steam-space of the boiler is increased by adding a vertical cylinder alongside the boiler, open to it. On the bottom of that vessel the steam might condense—pure water—and the temperature of the steam immediately over this water would be that of saturated vapour at the same temperature, and from there all through the steam-space to the surface of the saline solution in the boiler the temperature would increase, and all would have the same pressure. There would be mechanical equilibrium, but not thermal equilibrium.

"At the upper limit, *all* the heat is received at the highest point of temperature,"—but just as it would be if the evaporation were from a film of water upon a nearly bare combustion chamber crown. The plate is left in the boiler, and so is the salt, and in neither case would the steam exhibit a "state of things strongly contrasted with that which obtains when vapour rising from pure water is afterwards superheated." I have stated in my previous letter that the heat of evaporation is all *received* at identically the same temperatures as when it is raised from pure water at the same pressure, and the contrast is only as strong as that between occult and obvious. I have now shown that the vapours must be identical from either beginning; and unless each carried a certificate of birth, I do not now see how it would be possible to tell one from the other.

London, March 12. J. MACFARLANE GRAY.

Phoronomy.

I THINK it will be admitted by all, that precision of language is of great importance in scientific terminology; and the letter of Dr. Besant, which appeared in your issue of last week (p. 462), certainly suggests strong reasons for employing the word *phoronomy* in the place of *kinematics*.

The word may at first sight appear strange to the present generation of mathematicians; but if it becomes acclimatized, its employment will appear as natural as the phrases *kinetic* and *potential* energy, in the place of such meaningless phrases as *vis-viva* and *force-function*.

When the medical profession require a new word, they almost always have recourse to the Greek language; and mathematicians and physicists would do well to follow their example, and in cases of doubt or difficulty to consult some eminent classical scholar. I must confess, that I have no sympathy with the attempts, which have occasionally been made, to introduce short words of Teutonic origin into scientific nomenclature, as such words have always appeared to me to be singularly deficient in point. A good example of this is furnished by the word *spin*, which Clifford attempted to introduce in the place of the phrase *molecular rotation*. The latter phrase, although a little long, exactly expresses the idea which it is intended to convey, viz. that the molecules of the fluid possess a motion of rotation as well as a motion of translation. The word *spin*, on the other hand, does not express any such idea, but is strongly suggestive of the juvenile, though not altogether unscientific, pastime of spinning peg-tops. A. B. BASSET.

322 Oxford Street, W., March 18.

I HAVE before me the second edition of F. Redtenbacher's "Principien der Mechanik und des Maschinenbaues" (Mannheim, 1859), of which the first section is entitled "Die Bewegung als Erscheinung (Phoronomie)." Whether the term occurs already in the first edition (1852), I cannot affirm, but I remember very well that Redtenbacher, in his lectures in Carlsruhe, in 1858, insisted upon that term being distinct from "Dynamik" and "Kinematik." I conclude, therefore, that the majority of the 786 students of that year—among them many foreigners—as also those of other years, were conversant with the term.

M. AM ENDE.

Westminster Chambers, 5 Victoria Street,
London, S.W., March 19.

SOME other correspondent is pretty sure to be mentioning that Mr. W. H. Besant will find in Kant's *Metaph. Anfangsgründe der Naturwissenschaft* all the authority he could desire for his proposed use of the word "phoronomy." Kant regularly uses the word in the sense of the later "kinematic"; and he was man of science enough to justify anyone in following his lead.

G. C. R.

The Tudor Specimen of Eozoon.

IN reference to the remarks made by Sir J. W. Dawson (NATURE, March 17, p. 461) on my paper on the Tudor specimen of Eozoon (*Quart. Journ. Geol. Soc.*, vol. xlvii. pp. 348–55), I should like to say that the whole point of that paper was that it was based on Sir J. W. Dawson's original type. The figure of this specimen has been repeatedly republished by Sir J. W. Dawson, and, in the absence of illustrations or details of other specimens from Tudor, upon its evidence alone rests the asserted occurrence of Eozoon in the Tudor limestone, and the great claims based thereupon. The value of other specimens from this locality was not rated very highly by Sir J. W. Dawson so recently as September 1888, when he remarked, "*Without additional specimens*,[1] and in the case of creatures so variable as the Foraminifera, it would be rash to decide whether

[1] And he previously refers only to "*the* specimen," "this very interesting specimen," "the fine specimen from Tudor," &c.

the differences above noted[1] are of specific value."[2] I may add that I have recently seen the specimens of Tudor limestone exhibited in the Peter Redpath Museum, and my estimate of their value coincides exactly with that of Sir J. W. Dawson in 1888. As Sir J. W. Dawson most kindly promises his assistance to other workers, perhaps he would submit to some of them any specimens from Tudor which he regards as more conclusive than his original type.

It would seem rather unnecessary for anyone to trouble to infer from my paper that Sir J. W. Dawson has "regarded the Madoc and Tudor specimens as 'Lower Laurentian,'" when that is so directly stated by Sir J. W. Dawson in his description of his figure; viz. "Specimen of *Eozoon canadense* embedded in a dark-coloured homogeneous limestone occurring in the Lower Laurentian series at Tudor, Canada West" (*Quart. Journ. Geol. Soc.*, vol. xxiii. p. 265). J. W. GREGORY.

British Museum (Natural History), S.W.

The Theory of Solutions.

IN his last letter (NATURE, March 3, p. 415) Prof. Ostwald repeats his opinion that a theory is "a complex of laws, grouped around and derived from a main law," and infers from my letter that what I term a *theory* he would term an *hypothesis*.

If this were the whole point at issue, I could meet it in no better way perhaps than by referring Prof. Ostwald to his own works. For example, in his "Outlines of General Chemistry," are to be found not only numerous instances of the use of the word theory in its ordinary and accepted sense (*e.g.*, p. 58) but also cases in which it is employed as synonymous with hypothesis (*e.g.*, p. 187).

With regard to the definition of solutions as mixtures, Prof. Ostwald maintains that even if hydrates are formed in a solution, the solution is finally a mixture of the hydrates and the remaining solvent. The real question involved is unaffected by this explanation. There is no doubt whatever that to the majority of readers the definition, without any qualifying clause, that solutions are mixtures leads to one conclusion and no other—namely, that between solvent and dissolved substance there is no interaction of a chemical nature. Prof. Ostwald has in his letters stated that in some cases he considers such interactions occur; he has also stated that between chemical and physical processes he knows of no distinction. The definition is at variance with both these views, and it seems but fair to conclude that such discordant statements tend in no way to obviate that misconception which Prof. Ostwald so often deplores.

In defence of the application of van der Waals's equation to solutions, a process questioned by me in my letter, Prof. Ostwald states that van der Waals himself has taken up this very question. The method by which van der Waals approaches the subject, curiously enough, furnished the main grounds for my objections. The most superficial comparison of the complex formula which van der Waals deduces for a mixture of two substances, with such an application of his simple gas equation to a solution as that given in Prof. Ostwald's book, is ample justification for my strictures. But apart even from such evidence as to the inadequacy of the application, the form which it is finally made to assume is in itself a proof of its incompleteness. By judicious simplification the application is made to take the shape of a linear equation in which "pressure forces due to the interactions of molecules are absent." That is to say, the cohesion of solvent and dissolved substance, and the mutual reactions of both, are alike ignored. Further comment on such a method of accounting for the phenomena of solutions appears to me to be superfluous. J. W. RODGER.

London, March 7.

The Limpet's Strength.

THE limpet experiments of your esteemed correspondent, Mr. Percy Aubin, as reported in NATURE of March 17 (p. 464) would have been still more interesting and instructive had he weighed the animals deprived of their shells.

On April 10, 1890, I published my experiments showing that the shell-less limpet pulls 1984 times in the air its own weight, and about double when immersed in water.

Fasting fleas on an average pull 1493 times their own dead weight.

Other experimenters give the pulling power of the shell-deprived *Venus verrucosa* of the Mediterranean, a cockle-like creature, at 2071 times the weight of its own body.

The force required to open an oyster appears to be 1319·5 times the weight of the shell-less oyster.

J. LAWRENCE-HAMILTON, M.R.C.S.

30 Sussex Square, Brighton, March 19.

Technical Education for Novelists.

AMIDST the many schemes for technical education, could you not put in a plea for the "author of the popular novel"? Perhaps the need will best appear from these illustrations taken from the first 100 pages of a recently published and loudly heralded work.

(1) Scene—Kinder Scout, Derbyshire. Date—"after the snows and rains of early April," 1864. Time—*after* 8 *p.m.* "It was a clear, frosty night, *promising* a *full* moon."

(2) Same place, *Easter Eve*, 1864. "The wooded sides of the great moor were fading into dimness, and to the east a *young* moon was *rising*." W.

March 12.

THE ORIGIN OF THE YEAR.

I.

IT would seem that in the dawn of civilization it was not at all a matter of course that the sun should be taken as the measurer of time, as it is now with us; and in this connection it is worth while to note how very various the treatment of this subject was among the early peoples. Thus, for instance, it was different in Egypt from what it was in Chaldæa and Babylonia, and later among the Jews. In the Egyptian inscriptions we find references to the moon, but they prove that she occupied quite a subordinate position to the sun; while in Chaldæa it would seem that the moon was the chief thing worshipped, and it was thus naturally the chief means used for measuring time, and, so far as months were concerned, this, of course, was quite right. In Chaldæa, too, where much desert travel had to be undertaken at night, the movement of the moon would be naturally watched with great care.

An interesting point connected with this is that, among these ancient peoples, the celestial bodies which gave them the unit period of time by which they reckoned were practically looked upon in the same category. Thus, for instance, in Egypt the sun being used, the unit of time was a year; but in Chaldæa the unit of time was a month, for the reason that the standard of time was the moon. Hence, when periods of time were in question it was quite easy for one nation to conceive that the period of time used in another was a year when really it was a month, and *vice versâ*. It has been suggested that the years of Methuselah and other persons who are stated to have lived a considerable number of years were not solar years but lunar years—that is, properly, lunar months. This is reasonable, since if we divide the numbers by 12 we find that they come out very much the same length as lives are in the present day.

There seems little doubt that the country in which the sun was first definitely accepted as the most accurate measurer of time was Egypt.

"The Egyptians," says Ranke in the first chapter of his "Universal History," which is devoted to Egypt, "have determined the motion of the sun as seen on earth, and according to this the year was divided, in comparison with Babylon in a scientific and practically useful way, so that Julius Cæsar adopted the calendar from the Egyptians and introduced it into the Roman Empire; the other nations followed suit, and since then it has been in general use for seventeen centuries. The calendar may be considered the noblest relic of the most ancient times which has influenced the world."

[1] *i.e.* between *the* specimen from Tudor and those from other localities.
[2] "Specimens of *Eozoon canadense*," Mem. Peter Redpath Mus., 1888, p. 43.

A study of the Egyptian monuments has shown most conclusively that towards the end of the ancient empire the Egyptians possessed a year as accurate for calendar purposes as our own, and that they had been led up to the knowledge of its true length by successive steps.

As we shall show further on, this earliest of all years that we know of in history began at the summer solstice. Since one of the oldest temples at Thebes is oriented to sunset at the summer solstice, we should be not at all surprised if investigation shows that when that temple was built, more than 3000 years B.C., the Egyptian year really began in what we should call our summer. We have ample evidence of this. And I think there is little doubt also that when Stonehenge was built it certainly was built by people who began their year with the summer solstice, which is the time of the year in which in many countries it is the habit still to light fires upon hills and so on. If we look up the records of the peoples that lived, say, during the 1000 years preceding the birth of Christ, we find that the different races began their year at different times, and even that the same race at different times began their year differently; the choice lay among the equinoxes and the solstices.

Wherever the ancient Egyptians came from, whether they brought their year with them or invented it in the Nile valley, there is a belief that it at first consisted of 360 days only, that is 5¼ days too little. It is more likely that they brought the lunar month with them, taking it roughly as 30 days ($30 \times 12 = 360$), than that they began with such an erroneous notion of the true length of the solar year, seeing that in Egypt, above all countries in the world, owing to the regularity of the inundation, the true length could have been so easily determined, so soon as that regularity was recognized.

Beginning with the inundation (summer solstice) we have—

(1) The season or *tetraemne* of the inundation,
(2) ,, ,, ,, sowing,
(3) ,, ,, ,, harvest.

From the earliest times the year was divided into twelve months, as follows:—

Inundation ...	Thoth ...	End of June (Gregorian).
	Phaophi	,, July.
	Athyr	,, August.
	Choiak	,, September.
Seed time ...	Tybi	,, October.
	Menchir	,, November.
	Phamenoth ...	,, December.
	Pharmouthi ...	,, January.
Harvest ...	Pachons	,, February.
	Payni	,, March.
	Epiphi	,, April.
	Mesori	,, May.

Now whether the Egyptians brought their year with them or invented it in the Nile valley, there is a belief that it at first consisted of 360 days only, that is 5¼ days too little. It is more likely that they brought the lunar

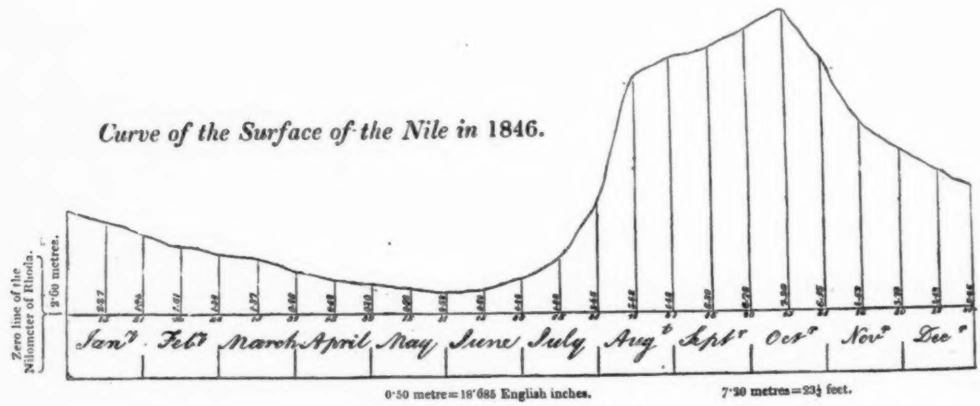


FIG. 1.—The annual rise and fall of the Nile (from Horner).

from a region where the moon was the time-measurer of not, so soon as they settled in the valley where the Nile then as now like a pendulum slowly beat the years by its annual overflows at the summer solstice, the solar basis of their calendar was settled.

We can well understand, therefore, since the whole life of the country depends upon the river, and all the energies of the inhabitants are connected with the work to be done during its rise and fall, that the moment of the commencement of the inundation, about the time of the summer solstice, should be chosen as the beginning of the year. Hence the perpetual reference to Solstice and Nile flood in the Egyptian annals.

It might be imagined at first sight that, as the year was thus determined, so to speak, by natural local causes, the divisions or seasons would be the same as those which Nature has given *us*. This is not so. Egypt is too near the tropics, and the local conditions are too different from our own, to permit of the application of our seasonal divisions of the year.

As Egypt, in the description quoted by Krall, "first appears like a dusty plain, then as a fresh-water sea, and finally as a bed of flowers," so the year is divided into three seasons instead of four.

month with them, taking it roughly as 30 days ($30 \times 12 = 360$), than that they began with such an erroneous notion of the true length of the solar year, seeing that in Egypt, above all countries in the world, owing to the regularity of the inundation, the true length could have been so easily determined, so soon as that regularity was recognized. We must not in these questions forget to put ourselves in the place of these pioneers of astronomy and civilization: if we do this, we shall soon see how many difficulties were involved in determining the true length of such a cycle as a year, when not only modern appliances, but all just ideas too, were of necessity lacking.

Still it is right that I should state that all authorities are not agreed as to the use of this year of 360 days. Ideler[1] considers it very doubtful. Krall,[2] however, urges that a certain inscription (the trilingual inscription of Tanis) expressly refers to it.

He adds to this some evidence, which he considers confirmatory, from religious usages. Thus at Philoe, in the temple of Osiris, there were 360 bowls for sacrifice, which were filled daily with milk by a specified rotation of priests. At Acanthus there was a perforated cask

[1] "Chronologie," i. p. 70.
[2] "Studien zur Geschichte des alten Aegypten," i. p. 16.

into which one of the 360 priests poured water from the Nile daily. Krall adds :—[1]

"It is probable that the year of 360 days dates from the time before the immigration into the Nile valley, when the Egyptians were unguided by the regular recurrence of the Nile flood. In any case, this must soon have convinced the priests that the 360-days year did not agree with the facts. But it is well known to everybody familiar with these things how long a period may be required before such determinations are practically realized, especially with a people so conservative of ancient usages as the Egyptians."

Supposing the use of this 360-day year to have been universal, it is perfectly certain that the Egyptians, now in this part of the Nile valley, now in that, must have got their calendar into the most hopeless confusion, compared with which "the year of confusion" was mere child's-play, and that the exact determination of the times of sowing, reaping, &c., by means of such a calendar would have been next to impossible.

As each year dropped $5\frac{1}{4}$ days, it is evident that in about seventy years $\left(\frac{365{\cdot}25}{5{\cdot}25}\right)$ a cycle was accomplished, in which new year's day swept through all the months. The same month (so far as its name was concerned) was now in the inundation time, now in the sowing time, and so on. Of fixed agricultural work for such months as these there could be none.

It must have been, then, that there were local attempts to retain the coincidences between the beginning of the year and the Nile flood and solstice ; intercalation of days or even of months being introduced, now in one place, now in another ; and these attempts, of course, would make confusion worse confounded, as the months might vary with the district, and not with the time of the year.

That this is what really happened is, no doubt, the origin of the stringent oath required of the Pharaohs in after times, to which I shall subsequently refer.

This year of 360 days had naturally to give way, and it ultimately did so in favour of one of 365. The precise date of the change is not known, but it is referred to in inscriptions of the time of Amenemha I. (*circ.* 2400 B.C.). This, of course, does not exclude the possibility, indeed even the probability, that it was introduced much earlier. The five days were added as epacts or epagomena ; the original months were not altered, but a "little month" of five days was interpolated at the end of the year between Mesori of one year and Thoth of the next.

When the year of 365 days was established, it was evidently imagined that finality had been reached ; and mindful of the confusion which, as we have shown, must have resulted from the attempt to keep up a year of 360 days by intercalations, each Egyptian king on his accession to the throne bound himself by oath before the priest of Isis, in the temple of Ptah at Memphis, not to intercalate either days or months, but to retain the year of 365 days as established by the Antiqui.[2] The text of the Latin translation preserved by Nigidius Figulus, cannot be accurately restored. Only thus much can be seen with certainty.

To retain this year of 365 days then became the first law for the king, and indeed the Pharaohs thenceforth throughout the whole course of Egyptian history adhered to this year, in spite of their being subsequently convinced, as we shall see, of its inadequacy for a long period. It was a Macedonian king who later made an attempt to replace it by a better one

The years of 360 and 365 days to which we have so far referred are termed in the inscriptions the "little" and "great" years respectively.

[1] *Loc. cit.*, p. 20.
[2] Mommsen, "Chronologie," p. 258.

How, then, was this 365-day year, which had been introduced with such pomp and circumstance, regulated? This brings us to a new point.

The Heliacal Rising of Sirius.

I have insisted upon the perfect regularity of the rise of the Nile affording the ancient Egyptians, so soon as this regularity had been established, a nearly perfect way of determining the length of the year.

It is also clear that so soon as the greatest northing and southing of the sun rising or setting at the solstices had been recognized, and that the intervals between them in days had been counted, a still more accurate way would be open to them, especially if, as I believe, the observations of the solsticial risings or settings were made in temples (or observatories) accurately oriented to the proper amplitude.

In this way, then, the great natural festival of the year would be the nearly coincident commencement of the inundation and the summer solstice.

As I have said, the solstice might have, one may say *must* have, occurred with greater regularity than the rise of the river, so that as accuracy of definition became more necessary the solstice would be preferred. The solstice was common to all Egypt, the commencement of the inundation was later as the place of observation was nearer the mouth of the river.

Now it seems as if among all ancient peoples each sunrise, each return of the sun—or of the sun-god—was hailed, and most naturally, as a resurrection from the sleep—the death—of night : with the returning sun, man found himself again in full possession of his powers of living, of doing, of enjoying. The sun-god had conquered death, man was again alive. Light and warmth returned with the dawn in those favoured eastern climes where man then was, and the dawn itself was a sight, a sensation, in which everything conspired to suggest awe and gratitude, and to thrill the emotions of even uncivilized man.

What wonder, then, that sunrise was the chief time of prayer and thankfulness? But prayer to the sun-god meant, then, sacrifice, and here a practical detail comes in, apparently a note of discord, but really the true germ of our present knowledge of the starry heavens which surround us.

To make the sacrifice at the instant of sunrise, preparations had to be made, beasts had to be slaughtered, and a ritual had to be followed ; this required time, and a certain definite quantity of it ; to measure this, the only means available then was to watch the rising of a star, the first glimmer of which past experience had shown preceded sunrise by just that amount of time which the ritual demanded for the various functions connected with the sunrise sacrifice.

This, perhaps, went on every morning, but beyond all question the most solemn ceremonial of this nature in the whole year was that which took place on New Year's morning, or the great festival of the Nile-rising and summer solstice, the 1st of Thoth.

How long these morning and special yearly ceremonials went on before the dawn of history we, of course, have no knowledge. Nor are the stars thus used certainly known to us ; of course any star would do which rose at the appropriate time before the sun itself, whether the star was located either in the northern or southern heavens. But in historic times there is no doubt whatever about the star so used. The warning-star watched by the Egyptians at Thebes, certainly 3000 B.C., was Sirius, the brightest of them all, and there is much evidence that Sirius was not the star first used.

"Besides the solstice and the beginning of the Nile flood, there was an event in the sky which was too striking not to excite the general attention of the Egyptian priesthood. We also know from the newly-discovered

inscriptions from the ancient empire that the risings of Orion and Sirius were already attentively followed and mythologically utilized at the time of the building of the pyramids."[1]

J. NORMAN LOCKYER.

(*To be continued.*)

THE WINTER STORMS OF NORTHERN INDIA.

THE physical constitution and history of the storms of India and Indian Seas is a subject which, almost from his first association with the Indian Meteorological Department, Mr. Eliot has made peculiarly his own. Besides nine elaborate, and, as far as possible, exhaustive, memoirs and reports on the history of particular storms, two on the tracks and periodicity of the cyclones of the Bay of Bengal during the ten years 1877–1886, and an admirable hand-book, in which he has summarized, for the guidance of seamen, the characteristic features and behaviour of these storms, his annual reports on the meteorology of India have always been replete with the results of his studies of the storms of the current year; and to him is mainly due that development which has been effected in the system of storm warnings for the coasts of India in recent years, and has rendered it one of the most efficient and comprehensive organizations for that purpose now in operation in any part of the world.

Like most other features of the climate, the storms of India differ very greatly in their leading characteristics at different seasons of the year. We have, in the first place, the well-known cyclones and cyclonic storms of the Bay of Bengal and the Arabian Sea, which are most frequent when the summer monsoon is at its height, and most severe at its commencement and termination. These are generated over some part of the tropical sea north of latitude 6°, and travel, as is now well known, on tracks between west and north—most frequently north-west or west-north-west; sometimes, however, in the spring and later months of the year, recurving to north-north-east or even north-east, as they approach the tropic. In virtue of their severity and destructiveness, these storms have attracted far more attention than any others, and not only Piddington's, but also the writings of most of his successors, have been almost exclusively devoted to them.

Of a very different type are the storms that bring the rainfall of the cold season and the earlier spring months to Northern India. It would be incorrect to speak of these as the storms of the winter monsoon (unless the term be understood, in its strictly etymological sense, as merely the name for a season of the year), for during their passage the northerly winds are suspended over a great part of India, and, with the rarest exceptions, they never penetrate far into the tropics. These storms, if they travel, always move from west to east. They have the usual cyclonic constitution, but the winds have but little force; and it was not until the preparation of daily weather charts for the first time showed their true nature, that this fact was even suspected.

Many of the features of these cold-weather storms are very striking and characteristic, and, as has been remarked by Mr. Eliot in his reports for 1888 and 1889, the temperature of Northern India in the cold season is chiefly determined by their number and character. Each of them is preceded by a wave of high temperature, and followed by a cold wave; except, indeed—and the exception is instructive—when the course of the storm is so far south of the Himalayas that little or no snow falls on the mountains (see "Climates and Weather of India," p. 206). In these cases, which are, however, exceptional, the cold wave is sometimes evanescent. On the other hand, as Mr. Eliot remarks, "the intensity and period [of the cold wave] largely depend on the amount of rainfall in Northern India and of the snowfall on the Himalayan mountain regions, and the height to which the snow-line has descended." As a rule, therefore, the cold wave follows the storm. Mr. Eliot gives, in his report, a table of the changes of temperature from day to day from January 20, to February 7, 1889, which includes two very characteristic illustrations of this phenomenon, and which therefore we extract. The figures show the variations of the observed mean temperature of each day from the average of many years for the same day. The crest of each warm wave is emphasized by strong type and the trough of each cold wave by italics.

The history of these two storms is as follows, and it exhibits one or two remarkable and suggestive features, which will presently be noticed more particularly. The first disturbance originated (or made its first appearance in India) on January 22. There were two separate centres and areas of disturbance, one of which covered the Punjab Himalaya and adjacent plains from Sialkote to Roorkee. This filled up [apparently] on the 23rd, after giving moderate snow on the hills and light showers on the adjacent plains. The other originated in Rajputana, and advanced, on the 23rd and 24th, in an easterly direction, across Northern India into Burma. It gave moderate general rain to the North-West Provinces and Central India, and light showers to Behar, Bengal, and Assam. This first storm was therefore of very moderate intensity. The snowfall and rainfall were but slight, and in the Punjab, Bengal, and Burma insufficient to bring down the temperature below the normal average.

The second disturbance was one of greater intensity, but like its predecessor had a double centre. One part consisted of a shallow depression, which passed into Sind from Baluchistan on the 28th, advanced through Central India, Behar, and Bengal, on the three following days, and into Burma on February 1, where it slowly filled up during the next three days. The other part was a deep depression, which formed in the Northern Punjab on the evening of the 28th, and during the next thirty-six hours marched slowly to the south-east along the southern face of the Punjab Himalayas. It filled up very rapidly on the evening of the 30th, and morning of the 31st of January, in the South-Eastern Punjab. The double disturbance gave a very heavy fall of snow over the whole of the Western Himalaya, bringing the snow-line down to 3500 or 4000 feet, and also general rain to nearly the whole of Northern and Central India, which was greatest in amount in the Punjab, the North-West Provinces, and Behar. As is shown by the table (see next page), the fall of temperature after this storm was proportionately great, amounting to 9½° in the Punjab, and to 14° or 15° on the mean of the day in Guzerat, Central India, and the Central Provinces. It continued three or four days after the weather had cleared up, so that the trough of the cold wave followed the crest of the warm wave after an interval of five or six days, and each occupied three or four days in passing from the Punjab to Burma. Mr. Eliot gives eight charts in illustration of these waves, of which we reproduce those for January 30 and 31 and February 1. They are projected for the observed temperatures at 8 a.m. of those days, and show, not the temperatures themselves, but the amounts by which these deviate in excess or defect from the averages of many years at the same hour. The isabnormals of deficient temperature are represented by broken, those of excessive temperature by continuous lines.

Mr. Eliot remarks that in the warm waves the greatest excess is generally exhibited by the night temperatures; and in a table which he gives of the deviations of the daily maxima and minima from their respective normal

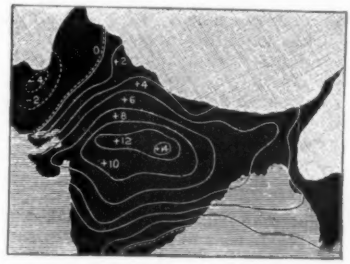
[1] Krall, *op. cit.*, p. 45. See also Brugsch, "Aeg. Zeit.," 1881, p. 1. *seqq.*

DAILY TEMPERATURE ANOMALIES (DEG. FAHR.).

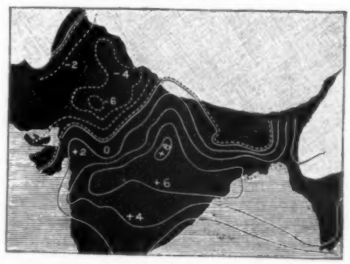
PROVINCES.	January 20th.	21st.	22nd.	23rd.	24th.	25th.	26th.	27th.	28th.	29th.	30th.	31st.	February 1st.	2nd.	3rd.	4th.	5th.	6th.	7th.
Punjab	−4·2	−0·5	**+4·0**	+2·3	*+0·1*	+0·2	+2·2	+2·9	**+3·2**	0	0	−2·2	−3·4	−3·5	*−6·1*	−4·3	−1·8	−0·6	+3·5
Sind and Rajputana	−4·1	−2·1	+2·8	**+5·2**	−0·6	*−0·9*	+3·2	+4·6	+4·1	**+4·4**	+3·6	−3·8	−1·6	−2·6	*−6·4*	−5·7	−0·7	+4·9	+5·4
Guzerat and Central India ...	−0·2	+1·4	+4·8	**+7·9**	*−1·8*	−0·8	+1·8	+4·8	+6·6	+9·5	**+10·9**	+2·6	−3·3	−2·3	+0·4	*−4·4*	−2·9	+1·8	+4·0
North-West Provinces	+2·5	+0·2	+0·9	**+5·3**	+4·3	*−3·2*	−0·8	−1·5	+1·7	+5·0	**+6·4**	+0·5	−1·5	−2·5	−4·1	*−5·2*	−5·0	−2·3	+1·0
Central Provinces...	+5·1	+4·7	+6·5	**+8·6**	+3·2	*−0·7*	+3·0	+5·6	+7·3	+9·7	**+10·4**	+6·0	−3·2	−3·1	+1·5	−1·1	*−4·1*	−0·7	+4·0
Bengal	+6·8	+2·9	+0·9	+1·3	**+3·4**	+1·8	+1·6	*+0·6*	+2·3	+3·9	**+6·1**	+4·2	+3·9	−2·4	−3·2	−2·4	*−5·0*	−4·2	−2·6
Burma	+2·0	+1·5	+1·4	+1·8	+2·5	**+2·6**	+2·5	*+1·4*	+1·7	+2·2	+2·6	+3·1	**+4·0**	+3·7	+1·3	+0·6	+0·4	*−1·5*	−0·1

values, it appears that those of the latter are from one and a half times to nearly twice as great as those of the former. On the other hand, in the cold waves which follow the storm, the day temperatures frequently show a greater relative depression than those of the night-time, indicating therefore that the nocturnal radiation under a clear sky is far from being the only cooling agency operative.

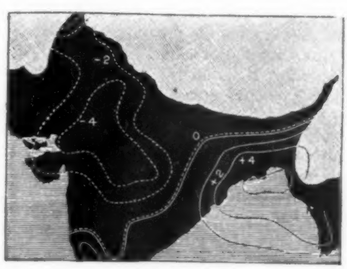
Notwithstanding this latter feature, the cold waves often bring about an inversion of the normal temperature relations between the hills and the plains, and that in a very remarkable degree. This was observed on no less than eleven nights in January 1889; and a very interesting paper on the subject was contributed by Mr.



January 30.



January 31.



February 1.

Temperature isabnormals of twenty-four hours preceding 8 a.m.

Eliot to vol. lix., Part ii., of the Journal of the Asiatic Society of Bengal. On one occasion, the figures of which are given, not only was the minimum night temperature of the hill stations (at elevations between 6000 and 8000 feet) from 8° to 13° higher than on the adjacent plains, but actually higher than anywhere on the Indo-Gangetic plain except only the coast districts of Bengal.

Before noticing the barometric features of these storms, let us see what is the probable explanation of their striking vicissitudes of temperature. The weather charts and reports

show that for a day or two previously to the appearance of the storm-centre in North-Western India light southerly or easterly winds prevail over Northern India, accompanied with increasing but not dense cloud, and a steady rise of vapour tension, which, as Mr. Eliot points out, is only in part explained by the increase of temperature. The distribution of the cloud is well marked and characteristic. "An examination of a large number of these storms shows that they invariably give light cloud to the south-east of their tracks, moderate cloud to the east, and overcast skies to the north. In fact the largest development of cloud usually occurs in the northern and north-eastern portion of the depression, and at a considerable distance to the north of the depression, as indicated by the isobars, . . . and usually in the hill districts, and adjacent plain districts of Northern India. Hence the cloud distribution appears to indicate that the ascensional motion accompanying these storms commences to the south, and proceeds slowly in the eastern quadrant, and is completed in the northern quadrant. The cloud thins off rapidly in the north-west quadrant, and in the west and south-west portions of the depression the skies are clear." These observations are very important; they show that the damp, warm air of southerly origin is absolutely restricted to the eastern side of the depression, for it is, so to speak, hall-marked by its freight of vapour, and this manifests its presence, and also indicates the region of its ascent and withdrawal from the earth's surface, by forming a thick cloud canopy, on the north and north-east chiefly. This too, of course, is the region of heaviest rain and snow fall.

The wave of warmth that precedes the depression is thus clearly explained by the unseasonable replacement of northerly by southerly winds; and since the warmth depends not only on the derived temperature of the air, but also, and in a very great measure, on the check given to nocturnal radiation by cloud and vapour, while it is not dense enough greatly to obstruct the solar rays, it is sufficiently obvious why the night temperatures should show a greater excess than those of the day-time.

For the reduction of the temperature in the cold wave that follows the depression, three distinct causes may be assigned: viz. the contact of a snow surface down to low levels on the Himalaya and Afghan mountains, from which are drawn the north-west winds of the western half of the storm; the evaporation of the rain that has fallen on the plains, and is rapidly taken up by the very dry air that has descended from the mountains; and lastly, the increased nocturnal radiation in this dry atmosphere under a cloudless sky. Of these it would seem that the first and second are the most influential. This seems to be indicated by the facts already quoted—that the intensity of the cold largely depends on the rain and snow fall, and the level to which the latter descends,[1] also that when the storm centre lies so far south of the mountains that little or no snow is precipitated on them, the cold wave does not always follow. It is further supported by the fact noticed by Mr Eliot, that the relative depression of the day temperature in the cold wave is frequently greater than that of the night hours. This effect of the snow is the more remarkable, when we consider that, in descending to the level of the Indo-Gangetic plain, the air must, of course, be dynamically heated about $5\frac{1}{2}^{\circ}$ for each thousand feet of descent. Nearly all this heat must have been expended in melting the snow, since the air arrives at the level of the plains dry and cold; so much so, indeed, that the further cooling it undergoes, in consequence of the evaporation of the rainfall and nocturnal radiation, brings down the night temperature of the plains sometimes as much as 13° below that of mountain ridges at 7000 feet.

It remains to notice the barometric features of these storms, for, if less striking than the vicissitudes of temperature that accompany them, they afford matter of much interest, and suggest questions of a wider bearing than such as have reference merely to the local circumstances of the sub-tropical zone. The barometric depression of these winter storms is as a rule very small, and the gradients are very low. On the Indian weather charts the isobars are laid down for increments of one-twentieth of an inch; but it is seldom that the storm vortex, when launched on the plains of India, is encircled by more than one or two closed isobars, and sometimes its position can only be gathered from the figures in the accompanying table of the reduced barometric readings, or recognized by the oblique inflection of the wind arrows. Such was the case with both the January storms, noticed above, which travelled across India to Burma, and also the Punjab secondary depression of January 22, 1889. But that which formed in the Punjab on January 28, and which is described as a stationary storm, was of a much more pronounced character, and for three days determined the leading features of the distribution of pressure all over North-Western India, the isobars being only slightly modified by the travelling depression further south. Of a similar type was the storm at the end of January 1883, of which illustrations are given in the report on the meteorology of India for that year. This too appears to have lingered in the angle of the Punjab inclosed by the Himalaya and the Afghan mountains, and the question forces itself upon us, whether there is not something in the physical features of this part of the country that favours the development of such vortices, and detains them while the feebler and shallower depressions that form part of the same general disturbance. pursue their course eastward across the plains. Mr. Eliot remarks on the effect of the Himalaya in causing a forced ascent of the southerly vapour-bearing winds, thus localizing the maximum precipitation on the north and north-east of the depression; and it is possible also that the angle at which the mountain chains meet on the north of the Punjab, inclosing the plain of that province, may not be without its influence in favouring the development and detention of the vortex.

It is still very obscure what are the general causes that determine the appearance of these storms in India. At one time it seemed to me probable that their origin was to be sought for in the local conditions of India itself, and even now I see no reason to question that, as distinct vortices, very many, perhaps most, of them originate on the plains of India; especially in such cases as the succession of storms in the earlier part of January 1883, of which a brief description was given in the "Climates and Weather of India." Mr. Eliot, too, seems to hold a similar view, since he speaks sometimes of storms "forming" in Rajputana, &c., sometimes merely as "first appearing" in Sind, Rajputana, or the Punjab. But in some of these instances there is evidence that the appearance of the storm on the plains of the north-western frontier was preceded by stormy weather in Afghanistan, indicating that the disturbance had reached India from that country or Baluchistan; and Mr. Eliot distinctly identifies a storm that appeared in the Punjab on January 9, 1889, with one that had been experienced at Bushire at the head of the Persian Gulf, on the 7th and 8th.

In this matter, we must distinguish between the barometric depression, which appears like the trough of a great atmospheric wave of very great extent both in longitude and latitude, sweeping across the country from the westward, and the vortex, or in some cases vortices, which are, as a rule, merely local and subordinate features of the former. No doubt, it is the passage of one of these troughs that in all cases determines the formation of the

[1] Some of the lowest temperatures hitherto recorded in Northern India were those observed in the first week of February 1883, four or five days after a storm which covered with snow the plain around Rawulpindi, only 1700 feet above the sea; down to a level therefore unprecedented in the meteorological annals of India. See Report on the Meteorology of India in 1883; also "Climates and Weather of India," p. 204.

storm; but, except when the advent of a vortex can be distinctly traced to the highlands of the western frontier, it seems very likely that its development and duration are in some degree influenced by the local conditions of the land surface, such as have been already noticed in the case of the Northern Punjab; and its intensity would seem to be mainly dependent on the amount of snow and rain that is precipitated.

From what has been said above, the general resemblance of the winter storms of Northern India to those of our own latitudes will be sufficiently obvious. In their eastward movements, the localization of the rainfall, the contrasted temperature conditions of the opposite quadrants, and many other particulars, we may recognize their essential identity. But certain features which are more or less blurred in our European storms, in those that we are now dealing with stand out with much clearer definition; and they seem calculated to throw not a little light on the still vexed question of storm generation, and perhaps to reconcile some of the very conflicting views that now prevail on this subject. As Mr. Lewis Morris says of the old Greek myths—

"These fair tales which we know so beautiful
Show only finer than our lives to-day
Because their voice was clearer and they found
A sacred bard to sing them"—

so may we say of these manifestations of aërial action in India. The phenomena are similar in kind to those that pass before our own eyes, but they stand out, accentuated by the circumstances of the climate and country, with a clearness and prerogative emphasis that we may seek for in vain in the confused and kaleidoscopic weather phases of these latitudes. We read their meaning almost at a glance, as we gather that of a printed page, and have not laboriously to pick out and piece together the obscure facts that express it, as with painful effort we might decipher the faded and half-concealed characters of a monkish palimpsest. And by good fortune, we have at the head of the Indian Meteorological Department an accomplished mathematician and physicist, who appreciates to the full the rich opportunities of his charge, and who knows how to marshal and interpret his facts as well as record them.

HENRY F. BLANFORD.

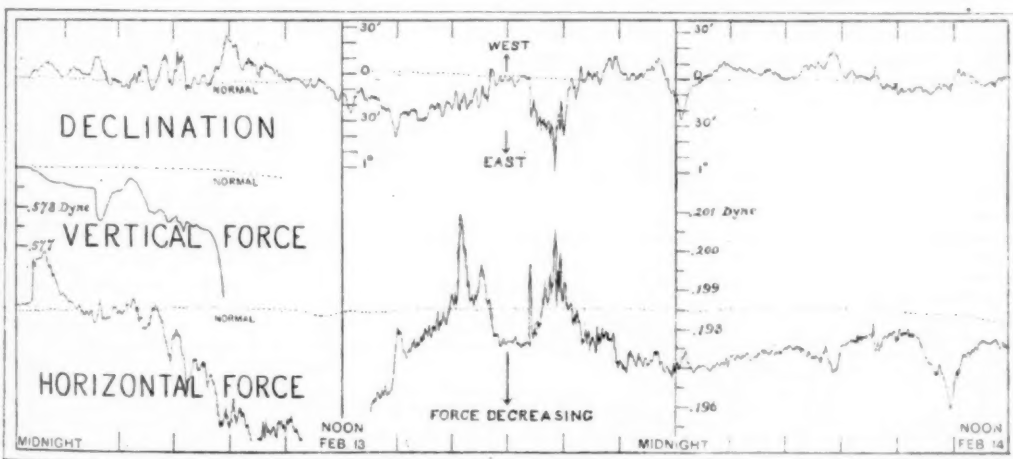
THE MAGNETIC STORM OF FEBRUARY 13-14, 1892.

THE Superintendent of the U.S. Naval Observatory sends us the following records of the magnetic storm of February 13:—

"The records of this unusually severe magnetic storm are of especial interest as occurring at the same time as the fine displays of auroræ and the appearance of a large group of sun-spots.

"The first increase in the horizontal force was followed by a rapid decrease, the force falling to much less than its usual strength, with rapid changes. Its change during the storm was 2¼ per cent. of its mean strength. The vertical force decreased so much that the sensitive balanced magnet used to record it was upset at 8 p.m. of the 13th, and its further record lost.

"The auroræ were seen at Washington at about 2 a.m. and 7.30 p.m. of the 13th, the latter time being marked by an unusually disturbed condition of the magnets."



"The magnetic storm commenced suddenly at 12.40 a.m. (75th meridian time), February 13, with a movement of the north end of the declination magnet to the westward, and a rapid increase in the horizontal and decrease in the vertical components of the earth's magnetic force.

"The north end of the declination magnet remained to the westward of its normal position until 10.30 a.m., when it crossed to the eastward, all the time oscillating violently, and did not return to the normal until 8 p.m. of the 13th, after which it kept oscillating on each side of its mean position until the end of the storm. It registered a change of direction of 1½°.

WILLIAM DITTMAR.

ALL who take an interest in the progress of chemistry will regret the death of William Dittmar; a smaller circle feel that they have lost an invaluable friend. Born at Umstadt, near Darmstadt, April 15, 1833, he was the second son of Fritz Dittmar, then Assessor at Umstadt, afterwards Landrichter at Ulrichstein, in Ober-Hessen, where he took a part in the movements of 1848 displeasing to the Hessian Government, who removed him from office, allowing him to retire on a pension. At the age of sixteen, therefore, William became a resident in

Darmstadt, where his father spent his enforced leisure. He was apprenticed to the Hof-Apotheker in Darmstadt, and in due time passed his "Gehülfe Examen" with distinction. He had access to a good collection of books on chemistry and physics, which he eagerly read. He went as Gehülfe to Mühlhausen in Alsace, where he spent several years, and returning to Darmstadt passed the "Staats Examen" in pharmacy, passing in the first class. But the attraction of pure chemistry prevailed, and in 1857 he went to Heidelberg. Bunsen soon saw what kind of student he had got, and appointed him assistant in the laboratory. There he met Sir Henry Roscoe, who invited him to Manchester as his private assistant. On Roscoe's appointment as Professor of Chemistry in the Owens College, Dittmar went with him as assistant. In 1861 he became chief assistant in the Edinburgh University Chemical Laboratory under Prof. Sir Lyon Playfair. In 1869 he went to Bonn, where he acted first as *Privatdocent* and afterwards as Lecturer on Meteorology at the Agricultural College at Poppelsdorf. In 1872 he declined the Chair of Chemistry in the Polytechnic School at Cassel, preferring to return to Edinburgh to his old post in the University. Here he remained only a few months, accepting in 1873 the Lectureship on Practical and Technical Chemistry in the Owens College. Thence he removed to Glasgow to succeed Prof. Thorpe in the Chair of Chemistry in the Andersonian College. This office he held till his death, February 9, 1892. He died literally in harness. He lectured in the morning, but not feeling very well, went home in the middle of the day, and after a few hours' illness died at 11.30.

He was a Fellow of the Royal Society and of the Royal Society of Edinburgh. In 1887 the University of Edinburgh conferred on him the degree of Doctor of Laws. The Philosophical Society of Glasgow awarded him last year the Graham Medal for his investigation into the composition of water.

Dittmar was a man of great intellectual energy, which always took a practical turn, indeed it is rare to see a man so truly scientific in all the operations of his mind so free from speculation. Not that his imagination was unused, but so prominent before him was the practical result to be obtained, that it gave a character of reality to the whole process by which he sought to reach it.

His most important work was analytical. His great investigation into the composition of the specimens of sea-water collected by the *Challenger* Expedition is a masterpiece of judgment and skill, important not only for its results, but perhaps even more for its methods. We may mention also his determination of the atomic weight of platinum, his method for the analysis of chrome iron ore, his examination of the alkaline hydrates and carbonates, and the gravimetric determination of the composition of water. But he did not confine himself to analytical work. He published along with Kekulé a paper on oxymethylbenzoic acid, the first aromatic alcohol acid; and also while at Bonn obtained glutaric acid by the reduction of Ritthausen's glutanic acid. He did much excellent work in physical chemistry. We may mention the determination of the vapour-pressures of formate of ethyl and acetate of methyl, his work on the dissociation of sulphuric acid and on the relation of the composition of acids of constant boiling-point to the pressure under which they are distilled. He made the construction of the balance a subject of special study, and the balances constructed for him by Oertling and by Staudinger are models of convenience and accuracy.

But Dittmar was greatest as a teacher. Patient and careful, he helped his students where they needed help, and led them to think and work for themselves. He had no ambition to make his pupils analyzing machines; they had to understand all that they did. Gradually his great power as a teacher came to be appreciated, and latterly his laboratory was filled with enthusiastic pupils. Those whom he has trained are his real works on analytical chemistry; but others can learn much of his method from his admirable treatise on qualitative analysis.

A. C. B.

SERENO WATSON.

THE last American mail brought the sad intelligence of the death of this indefatigable botanist, upon whom, in one sense, the mantle of Asa Gray fell barely four years ago. Early in the year he was seized with a bad attack of *grippe*, and although he rallied and was better for a time, he never recovered strength, and finally succumbed on the 9th inst., in the sixty-sixth year of his age. Of his early life we know nothing, but he appears to have published no botanical work previous to 1873, about the period that he was appointed Herbarium Assistant to Dr. A. Gray at Harvard. From that date, however, onward until within a few months of his death, he was, next to Gray, the most active writer on North American Phanerogams. Much of his work appeared originally in the Proceedings of the American Academy of Arts and Sciences, under the title of "Contributions to American Botany," numbered consecutively, the last being the eighteenth. These consist principally of monographs of North American genera and descriptions of novelties. He was also the principal author of the "Botany of California," the last volume of which appeared in 1880; and since the death of Dr. A. Gray, he in conjunction with Prof. J. M. Coulter has edited the sixth edition of the deceased author's valuable "Manual of the Northern United States." This work has been considerably decried by contemporary American botanists, because Watson did not introduce the changes in nomenclature consequent on a strict and unqualified observance of the law of priority. But in this conservatism he doubtless followed the wishes of his former master, and enjoyed the sympathies of those whose experience teaches them that it is much easier to make these changes in books than to carry them into practice. Watson had a still more important work in hand, for he had undertaken the continuation of Gray's "Synoptical Flora of North America." How far this is advanced we do not know, but it is not probable that it will see the light on the same lines as the published volumes, or as he would have continued it. Unfortunately, an exceedingly useful work, commenced during the early part of Watson's engagement at Harvard, was never completed. We allude to his "Bibliographical Index to North American Botany," which was only carried to the end of the Polypetalæ. To a great extent, Gray's "Synoptical Flora" takes its place, so far as the Gamopetalæ are concerned; but it is difficult to find one's way in the remaining groups. Though Sereno Watson was of a retiring disposition, and did not belong to the teaching body, nor take a prominent part in the gatherings of scientific men, yet the loss of him will be widely felt and deplored. He was elected a Foreign Member of the Linnean Society of London in 1890, but he was not a man who craved after honours and distinctions.

NOTES.

IT seems almost incredible that the Treasury should think of stopping the publication of the *Kew Bulletin* simply because it does not quite pay its own expenses. The periodical, as our readers know, is one of high value, both from the scientific and the industrial point of view, and, if the Treasury persists in the design attributed to it, something ought soon to be said on the subject in Parliament by the scientific members. The *Times* has argued strongly against the proposed step, and the view it has expressed will be shared by all who are capable of forming

an intelligent opinion on the subject. It cannot be expected that a very large number of copies of the *Bulletin* will be sold, as it is really more useful to our colonial Governments than to individuals either here or in the colonies.

THE Council of the Royal Society of New South Wales has awarded to Mr. W. T. Thiselton-Dyer the Clarke Memorial Medal, in recognition of his distinguished services in the cause of botanical science, and especially on account of his labours in connection with the development and organization of the Botanical Departments for the Colonies and India, at the Royal Gardens, Kew. The medal has been forwarded with a letter, dated December 23, 1891, in which Mr. W. H. Warren, the Honorary Secretary, says:—"The Council fully appreciates the beneficial effects which this colony (in common with the other British possessions) has already derived and will continue to derive from the foresight and scientific zeal you have displayed in the building up of the Colonial Departments of your institution; the Council is also aware of the assistance which the Department under your direction has given to institutions in Sydney, and is not unmindful of the fact that the first collections obtained by the Sydney Technological Museum were received from the Museum of the Royal Gardens, Kew. The Council trusts that you will therefore accept the medal, as a token on the part of this Society of the appreciation in which your work is held in Australia." Mr. Warren's letter, with Mr. Thiselton-Dyer's answer, is published in the new number of the *Kew Bulletin*. In a prefatory note the Director of the Royal Gardens explains that in publishing the correspondence he feels "he is only putting on record a mark of appreciation as handsome as it is spontaneous, on the part of one of the most distinguished of the colonies of the Crown, of the usefulness of the official work which the Kew establishment could alone accomplish with the continuous and loyal assistance of every member of its staff."

STUDENTS of archæology will be glad to hear that Mr. F. C. Penrose has gone to Greece to carry on his investigation of the dates of Greek temples as derived from their orientation. He hopes to determine the orientation of many foundations not included in the list given in his recent paper on the subject. He will also verify, as far as possible, the approximate results at which he has already arrived.

ON March 28 many educational institutions in Austria and Germany will celebrate the three hundredth anniversary of the birth of Johann Amos Comenius, one of the most illustrious of pedagogues. The Austrian Government, however, has forbidden the proposed celebration in Bohemia. Comenius was by birth a Moravian. He anticipated many of the best ideas of our own time on education, and by his numerous writings and his great personal influence produced a profound impression on his contemporaries. Charles I. invited him to England to improve the organization of English schools; but the outbreak of the Civil War made it impossible for him to give effect to his ideas in this country.

PROF. GRIESBACH lately forwarded to Vienna various fossils which he had collected during his geological explorations in the Central Himalayas on behalf of the Government of India. They resemble so closely fossils found in corresponding Alpine strata, that they have excited much interest; and the Royal Imperial Academy of Science, Vienna, has determined, with the cooperation of the Indian Government, to send an exploring party to the Central Himalayas to compare their geological features with those of the Eastern Alps. The leader of the party will be Dr. Carl Diener, lecturer on geology at the University of Vienna. Dr. Diener is President of the Vienna Alpine Club, and is well known as the author of a work on the geological structure of the Western Alps. He will start for Brindisi on April 10, taking with him two Tyrol guides. The expedition will last six months.

THE anniversary meeting of the Chemical Society will be held on March 30 at 4 p.m.

DR. GEORGE BUCHANAN, F.R.S., who has long been known as one of the highest authorities on sanitary science, is about to erisgn the post of medical officer to the Local Government Board.

THE following are some of those who have consented to serve on the jury for the Crystal Palace Electrical Exhibition:—Prof. W. Grylls Adams, Prof. W. E. Ayrton, Mr. Shelford Bidwell, Prof. W. Crookes, Major-General Festing, Prof. George Forbes, Captain Sir Douglas Galton, Dr. J. H. Gladstone, Prof. D. E. Hughes, Mr. W. H. Preece, Prof. Silvanus Thompson.

ON Monday evening Mr. Kimber asked the Chancellor of the Exchequer why the British Government had not concurred with the other European Governments in joining the International Geodetic Bureau of Vienna. In reply, Mr. Goschen said the question of joining the reconstituted International Geodetic Bureau was raised just five years ago, the condition being an annual contribution of 2250f. a year for ten years, besides the expense of sending delegates to attend the meetings of the Bureau. "Our experience of the International Metric Bureau at that time," Mr. Goschen continued, "showed that the expenditure upon such undertakings tends to increase out of proportion to their actual utility, and it was considered that the practical advantages of joining the Geodetic Bureau were not sufficient to justify the guaranteeing of the sum named. So far as I am aware, the question has not been mooted since."

THE first annual meeting of the North-West of England Boulder Committee was held at the Technical School, Stockport, on the 19th inst. The year's work has been eminently practical and useful. The Committee, in addition to contributing a very large portion of the Report of the Erratic Block Committee of the British Association presented at Cardiff, have read and discussed more than forty papers and reports at their monthly meetings; these they now propose to print independently. Maps on the one-inch and six-inch scale have been acquired, partly by purchase and partly by presentation, including a valuable set from Sir A. Geikie, F.R.S., Director-General of the Geological Survey. On these good progress has been made, by a distinctive system of symbols, in showing the position and origin of the boulders over a large area. A thoroughly practical "Drift Primer" has been drawn up by the Secretary and approved by the Council of the Committee for the instruction of observers, and has circulated beyond the limits of the Committee. Boulder-forming rocks have been collected in England and Scotland, for reference purposes, and the nucleus of a Glacial Drift library formed. The Annual Report shows a large increase of members outside the original district of observation, and it was therefore decided that henceforth the title should be altered to "The Glacialists' Association," and that the rules be altered so as to include the whole of "the British Isles." The President, Mr. De Rance, and the Secretary, Mr. Percy Kendall, were re-elected, and the following Vice-Presidents were elected: Vice-Chancellor Sir Henry Bristowe, Mr. Brockbank, Mr. Gray, Alderman Kay, and Dr. Ricketts, and a Council of fifteen.

AMONG the contents of the new number of the *Kew Bulletin* is an interesting account of the Spanish Broom as a fibre plant. Some time ago a French scientific journal printed a notice respecting the use of the fibre of the Spanish Broom among the peasants in the neighbourhood of Lodéve, and in the remote hamlets in the mountains of Languedoc. An effort was made to secure speci-

mens of the fibre, and of articles produced from it, for the collections in the Museums of Economic Botany at Kew. It was with the utmost difficulty that specimens were obtained; but ample material for arriving at a definite conclusion with regard to the origin and character of "Genista fibre" was at last received. There is now in the Kew Museums a complete set, consisting of twigs, fibre in various stages of preparation, as well as yarns and coarse cloths. These were sent by Mr. Consul Perceval. There is also a sample of coarse sheeting received from M. Geoffroy St. Hilaire through the English Embassy at Paris. These fully illustrate the fibre industry connected with *Spartium* (Genista) *junceum*. "It is evident," says the *Bulletin*, "that this interesting rural industry is fast dying out in France. It may be said to exist now only in very remote hamlets in the Cevennes. The inquiries made by Kew were therefore only just in time to secure the last specimens of cloth made in the laborious fashion before the days of rapid communication and the introduction of cheap cotton and other goods."

ONE of the most interesting of recent additions to the Museums of Economic Botany at Kew has recently been received from Sir John Kirk. It consists of a large sheet of bark cloth prepared by the natives of Uganda from the inner bark of a species of *Brachystegia*, a small genus of trees belonging to the *Cæsalpineæ* sub order of the natural order *Leguminosæ*. Various details relating to the use of *Brachystegia* as a source of bark cloth are given in the current number of the *Kew Bulletin*. The same number contains sections on oil palm fibre and the sources of rubber supply.

ANYONE who may desire to devote himself to the study of Finnish archæology and folk-lore will find ample material for study in the information collected by the late Dr. H. A. Reinholm. He was chaplain of the prison, and pastor of the Lutheran congregation, at the Fortress of Sveaborg, near Helsingfors, and for many years devoted the whole of his leisure time to the amassing and arrangement of facts relating to the life of the Finnish people in past times. He died in 1883. Only a few results of his researches have been published. By far the greater part of his work is preserved in manuscript in the Historical Museum at Helsingfors. An interesting account of the labours of this indefatigable investigator is given in the current number of *Globus*.

MR. R. H. SCOTT delivered a lecture at the Royal United Institution on March 18, on a subject of much importance to meteorologists in this country, viz. "Atlantic Weather and its connection with British Weather." He pointed out that less than a quarter of a century ago, before synchronous charts were in vogue, it would have been impossible to have traced a storm across America and the Atlantic to our own coasts; but this can now be done with considerable certainty. The broad principles which govern the weather system of the Atlantic were shown on two diagrams exhibiting the mean pressure, and the regions of greatest disturbance of temperature, on the globe, in our winter. The latter chart showed that, at that season, the relatively warmest district is near Iceland; and the barometer chart showed that close to the same region the barometer is lowest. The reasons of these relations, which involve the first principles of modern weather knowledge, were fully explained. The more northern part of the Atlantic area interests us the most. The whole region from 40° to 70° N. is constantly visited by cyclonic depressions, and in order to throw some light on the origin and history of these depressions, and of the storms which they at times bring with them, various institutions have published daily maps of the weather in the Atlantic. The most complete of these maps were published by the Meteorological Office for thirteen months, commencing with August 1882. The last twelve of these months have been carefully examined, and show no less than 264 depressions in various parts of the ocean. Of these, out of 62 which originated south of 40° N., only 16 had sufficient energy in them to cross the meridian of Greenwich, while out of 22 which originated further south only 11 crossed the Atlantic, and these were not all felt as actual storms in this country. The practical outcome of obtaining telegrams from America has not been satisfactory, but this failure has probably been mainly due to the fact that the reports "have been neither numerous nor full enough." This accurately represents the case at the present time; but we hope it is not too much to expect that, with our present knowledge of the paths taken by depressions with regard to areas of high pressure, some further advance may shortly be made in predicting storms by means of more numerous and fuller telegraphic reports both from outward and homeward bound ships.

THE following are among the lecture arrangements at the Royal Institution for the period after Easter:—Prof. T. G. Bonney, two lectures on "The Sculpturing of Britain—its Later Stages" (the Tyndall Lectures); Mr. Frederick E. Ives, two lectures on "Photography in the Colours of Nature"; Prof. Dewar, four lectures on "The Chemistry of Gases"; Prof. H. Marshall Ward, three lectures on "Some Modern Discoveries in Agricultural and Forest Botany" (illustrated by lantern). The Friday evening meetings will be resumed on April 29, when a discourse will be given by Dr. William Huggins on "The New Star in Auriga"; succeeding discourses will probably be given by Captain Abney, Dr. B. W. Richardson, Mr. J. Wilson Swan, Sir James Crichton-Browne, Mr. Ludwig Mond, Prof. Dewar, and other gentlemen.

A GOOD seam of coal from 7 feet to 8 feet thick has been discovered by Mr. Hughes, of the Indian Geological Survey, on the banks of the Tenasserim River, which is navigable to that point. The Government of India has sanctioned the grant of a large concession in Mergui to Ah Kwi, a wealthy Chinese resident of the Straits, to prospect for tin. According to the Calcutta correspondent of the *Times*, this is the first attempt to encourage on a large scale the mining industry in Mergui.

MESSRS. CROSBY LOCKWOOD AND CO. have issued the fourth edition of Mr. Primrose McConnell's "Note-book of Agricultural Facts and Figures for Farmers and Farm Students." The author was originally induced to prepare the volume by noticing the great value of Molesworth's "Pocket-book of Engineering Formulæ" to engineers, and of similar books to those engaged in other professions. It occurred to him that a book compiled in the same style, and devoted to farming matters, could not fail to be useful as a ready means of reference for refreshing the memory. The success of the "Note-book" has proved that he was right. The progress of agricultural practice and science has been so rapid that it has been necessary for him to rewrite the greater part of the book, and nearly twice as much matter is given in the present edition as was contained in the earlier issues. The use of a slightly longer page and thinner paper has prevented the size of the volume from being much increased.

THE Agricultural Research Association for the north-eastern counties of Scotland has issued its annual report for 1891. It includes a valuable paper on "Root Hairs," in which Mr. T. Jamieson presents the results of a laborious investigation he has carried on during the past three years. He also gives some hints on permanent pasture, and brings together various items of information which are likely to be of immediate benefit to farmers.

IF we may judge from its twenty-sixth annual report, the American Society for the Prevention of Cruelty to Animals is

doing much good work. During the past year it prosecuted no fewer than 17,847 cases in the courts. Through its efforts 49,118 disabled animals were temporarily suspended from work; 34,264 horses, disabled past recovery, were humanely destroyed; 6444 disabled horses were removed from the streets in an ambulance. The number of prosecutions and other official interferences was larger than in previous years, but it does not follow that cruelty is more common than it was. The increase is due to the greater vigilance of the Society's officers.

In a paper contributed to the current number of the Journal of the Franklin Institute, Prof. Lewis M. Haupt argues strongly in favour of the construction of a ship canal between New York and Philadelphia, connecting the Delaware and Raritan Rivers. Such a canal would, he maintained, extend the Erie Canal and its benefits to Philadelphia, and open to its manufacturers over 16,000 miles of waterways in the great basin of the Mississippi. It would reduce the distance by water to New York harbour from 240 to about 60 miles, would afford an inside and safe passage to Eastern, Sound, and Hudson River ports; would develop a large population along the entire route, and so benefit the railroads traversing the district. "In short," says Prof. Haupt, "the effect would be to reduce the rate per mile, as well as to shorten the distance between the two greatest centres of population on the American continent, or, we may say, in the world; for nowhere else on the globe is it possible by so short and inexpensive a waterway to connect such large populations and so many and valuable interests."

The Bethlehem Iron Company, Pennsylvania, is to erect at the Chicago Exhibition a full-size model of its 125-ton steam hammer, said to be the largest in the world. It will span the main avenue of Machinery Hall, and will rise to a height of 90 feet. At the last Paris Exhibition great attention was attracted by a similar model shown by the Creusot works, but representing only a 100-ton hammer.

Baron von Mueller records, in the *Victorian Naturalist* for February, that, while elaborating diagnoses of new Papuan plants, he was pleasantly surprised to find among the novelties an Antholoma. This genus has hitherto been supposed to be restricted to New Caledonia. The Papuan species is dedicated to Prof. van Tieghem. The denticulation of the leaves, the elongation of the setule of the anthers and the three-celled ovulary already separate *A. Tieghemi* from *A. montanum.* Among the novelties are also *Oxalis* (*Biophytum*) *albiflora*, *Sloanea Forbesii*, which approaches *S. quadrivalvis* in many respects, but is petaliferous, and *Quintinia Macgregori* is particularly remarkable.

A "Treatise on Physical Optics," by Mr. A. B. Basset, will be issued shortly by Messrs. Deighton, Bell, and Co.

The proper title of Mr. A. E. H. Love's work (included in our list of forthcoming scientific books last week) is "A Treatise on the Mathematical Theory of Elasticity."

Messrs. Nalder Bros. and Co. have issued price lists, carefully illustrated, of their electrical testing and other scientific instruments, and of their ammeters and voltometers, resistance frames, &c.

Messrs. Dulau and Co. have issued Part xvii. of their "Catalogue of Zoological and Palæontological Books." It contains lists of works on Mollusca and Molluscoida.

In Mr. George S. Carr's letter on the terms "centrifugal force" and "force of inertia" (Nature, p. 463), in the second sentence of the second paragraph, read "in every case as the reaction to the normal component of the *centripetal* force" (not "*centrifugal*").

At the meeting of the Belgian Academy of Sciences on March 6, Prof. Spring announced that the late Prof. Stas had left, in an almost completed condition, a long and important memoir describing the results of several further stöchiometrical investigations. It is entitled "Silver," and will forthwith be edited, presumably by Dr. Spring, and published. It may be remembered that, after the publication of Prof. Stas's classical memoir upon the preparation of absolutely pure silver and the atomic weight of that metal, doubts were thrown by Prof. Dumas on the validity of the work on the ground that the silver employed was not free from occluded atmospheric gases. Moreover, Prof. Dumas expressed doubts as to the bearing of the work upon the celebrated hypothesis of Prout, according to which the atomic weights of all the other elements are supposed to be multiples of that of hydrogen. For, if silver possessed the atomic weight attributed to it by Prof. Stas, the atomic weight of oxygen became 15·96 and not the whole number 16, and consequently Prout's hypothesis in its original form would be negatived. In order to set these doubts at rest, and to leave his work in a perfected condition, Prof. Stas has prepared a quantity of silver with such extreme precautions that he has succeeded in obtaining it entirely free from occluded gases, and from even the minutest traces of the materials of the vessels employed. So perfect is the purity of this silver that even when heated to the temperature of the melting-point of iridium not a trace of sodium can be detected in the spectrum of the vapour. With this silver he has repeated his former determinations of the atomic weight of the metal, and it is satisfactory to learn that the final number obtained is, as Prof. Stas himself expected it would be, identical with that formerly obtained. Hence, the objection of Prof. Dumas cannot longer be entertained, and the atomic weight of oxygen would indeed appear to be 15·96 and not 16, for the numbers obtained by Prof. Stas agree so remarkably that an error of four-hundredths of a unit would apparently be out of the question. In addition to this important memoir, Prof. Stas has also left the data of a series of twelve separate determinations of the stöchiometric relation of silver to potassium chloride, the materials for which were the pure silver just described, and a specimen of potassium chloride, also prepared with a care and precaution quite in keeping with the rest of the work of the great analyst. The results of these determinations are described by Prof. Spring as agreeing in a most wonderful manner, and will afford another valuable base to which the atomic weights of many other elements may be referred. Besides these two memoirs, a third is mentioned by Prof. Spring, relating to the spectra of several metals which Prof. Stas has obtained in the purest state in which these metals have ever probably been seen. The whole of these memoirs, consisting of about fifteen hundred pages of manuscript, it is intended to publish forthwith in three separate treatises.

The additions to the Zoological Society's Gardens during the past week include a Ring-necked Parrakeet (*Palæornis torquatus* ♂) from India, presented by Mr. George H. Whitaker; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, presented by Miss Mildred Whitaker; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mr. J. S. Gibbons; a Nutmeg Fruit Pigeon (*Carpophaga bicolor*) from the Torres Straits, presented by Mrs. Fitzgerald; two Pike (*Esox lucius*) from British Fresh Waters, presented by Mr. P. F. Coggin; a Mantchurian Crane (*Grus viridirostris*) from North China, deposited.

OUR ASTRONOMICAL COLUMN.

Fuzziness of some Variable Stars.—Mr. Cuthbert G. Peek has, during the last six years, used his 6¼-inch achromatic for the investigation of the light-curves of variable stars. In

this month's *Knowledge* he describes some observations of changes in appearance of a few variable stars at different epochs. Three variables—T Cassiopeiæ, R Cassiopeiæ, and S Herculis—have been frequently observed as (*a*) remarkably well defined, almost planetary, disks; (*b*) well-defined stars, surrounded by a more or less dense, ruddy atmosphere; (*c*) large, woolly, ill-defined images, resembling a small but bright planetary nebula; (*d*) at minimum, in place of the variable, a slight bluish nebulosity. The changes appear to be real, for stars near the places of the variables have been seen clear and sharp when the haziness of the variables was unmistakable. Other stars with regard to which Mr. Peek has made similar observations are S Cassiopeiæ, R Tauri, R Aurigæ, V Cancri, R Ursæ Majoris, S Ursæ Majoris, R Camelopardi, R Boötis, S Coronæ, R Aquilæ, and S Cephei.

ASTRONOMICAL POSSIBILITIES AT CONSIDERABLE ALTITUDES.—Prof. Pickering, in No. 3079 of the *Astronomische Nachrichten*, relates some interesting facts in an article on "Astronomical Possibilities at Considerable Altitudes." They are gleaned from observations made at the Boyden Station of the Harvard College Observatory, which is situated two miles from the city of Arequipa, Peru, in latitude 16° 24′ S., and longitude 4h. 45m. 30s. W. of Greenwich, and at an altitude of 8060 feet above sea-level. The air there is so clear and steady that 6·5 magnitude stars are picked out by the naked eye with great ease, and, when the moon is not too bright, the eleven Pleiads can always be counted. The nebula in Andromeda forms also a very conspicuous object, "appearing larger than the moon," while, in the 13-inch Clark refractor, "the whole photographic region of the great Orion nebula, first shown in the Harvard photographs of 1887, is clearly visible to the eye," rendering it the "most splendid object in the stellar universe." The steadiness of the atmosphere is also very much remarked there, so much so that a scale of steadiness has been adopted. Some of the brightest stars have been noticed to have as many as six complete diffraction rings round them; while around these, when the seeing was denominated as "perfect," twelve rings have been counted. "Boiling" was also found to be sometimes completely eliminated, for, in observing bodies of the solar system with a 13-inch and a power of 400, "it was frequently impossible to detect any wavering of the edges of the disk."

The conclusion that Prof. Pickering comes to with regard to the position of future Observatories is that "moderate altitude is a most desirable qualification," while "for transparent skies one must approach the tropics, and for steady seeing one must have an extremely dry climate."

INCREASE OF THE EARTH'S SHADOW DURING LUNAR ECLIPSES.—In a memoir with the title "Die Vergrösserung des Erdschattens bei Mondfinsternissen" (*Abhandlungen der math. phys. Classe der k. Sachsischen Ges. d. Wissenschaften*, vol. xvii., Lepzig, 1891), Dr. Hartmann published the results of an investigation into the amount by which the earth's atmosphere increases the diameter of the section of the shadow during a lunar eclipse. An abstract of the memoir appears in the annual report of the Royal Astronomical Society, which has just been issued. Since the time of Tobias Mayer (1750) the coefficient $\frac{1}{60}$ has been assumed to represent this increase, although nothing is known as to the manner in which this quantity was determined. Dr. Hartmann has reduced all the observations of lunar eclipses observed independently by several astronomers during this century, and has deduced the increase of the diameter of the shadow from them. The result of a comprehensive discussion of 2920 observations of the contact of the shadow with well-defined lunar formations is, that the increase of the semi-diameter of the shadow is 48″·62 for mean lunar parallax. This corresponds to a coefficient of increase $= \frac{1}{50\cdot 79}$. The result may perhaps be changed 2″ or 3″ by a discussion of new observations, but not more, so it seems desirable that the value of $\frac{1}{50}$ should be used, when required, instead of Mayer's value of $\frac{1}{60}$.

THE NEW STAR IN AURIGA.

THE new star is rapidly getting more and more difficult of observation in consequence of its waning light. There is very little change in its spectrum, and what change there is is not in the direction recorded of Nova Cygni, so it seems pretty clear that the new body was not a hitherto unobserved nebula to begin with.

The *Astronomische Nachrichten*, No. 3079, contains (at p. 109) the following communication from Mr. H. C. Vogel, Director of the Astro-physical Observatory at Potsdam, dated February 29:—

"Although the spectroscopic observations of the Nova in Auriga are not yet concluded—since the star will probably continue visible for some time—I consider it of importance, in the interest of the subject, to communicate my observations made hitherto, and the conclusions drawn therefrom, even though the latter should not in the future be confirmed in all points.

"Concerning, first, the direct spectroscopic observations, I have, on February 20, observed the Nova with a compound spectroscope of a dispersion sufficient just to show the nickel line between the D lines. The hydrogen lines C, F, and Hγ appeared bright. Their identification was easy by means of a hydrogen tube in front of the slit. These three lines did not exactly coincide with the lines of the comparison spectrum, but were displaced considerably towards the red, without, however, separating completely from the artificial lines, since they were very broad. The continuous spectrum appeared faint, owing to the comparatively high dispersion; and with certainty only the dark broad F line was recognizable, situate towards the more refrangible side, distinctly separated from the bright line in the spectrum.

"Between C and F, a large number of bright lines could be seen, but most of them were too faint to be fixed with certainty. In the case of two brighter lines near F, myself and Mr. Frost, who assisted in the observations, succeeded in making very certain wave-length determinations; we found 492·5 $\mu\mu$ for the fainter of the two lines, which appeared broad and fuzzy on both edges, and 501·6 $\mu\mu$ for the brighter line. The limit of error is to be taken at about $\pm$ ·3 $\mu\mu$, and it results from the observation with certainty that the brighter line is *not* identical with the double line of the air spectrum or with the brightest line of the nebula, and still less the other with the second nebula line. From Young's list of lines most frequent in the chromosphere, it follows that near F only the two groups of lines, 501·87, 501·59, and 493·44, 492·43, 492·24, 491·92 frequently appear bright. There is no doubt that both lines in the spectrum of the Nova are chromosphere lines, and this result appears to me of great importance, in so far as it is made probable that the line observed in Nova Cygni (1876)—W.L. 500 $\mu\mu \pm 1\ \mu\mu$—which, during the gradual fading of the star, alone remained, was a chromosphere line, and not the nebula line.

"Further, both myself and Mr. Frost saw probably the magnesium lines, certainly the sodium lines bright, as also two lines between *b* and D, one of which probably was the well-known chromosphere line W.L. 531·72, also observed in Nova Cygni. By direct comparison with the hydrocarbon spectrum, the brightest band of which nearly coincides with the *b* group, and with the sodium flame, *b* and D were identified. Mr. Frost could see a displacement of the D lines in the star spectrum with respect to the comparison spectrum. There was no indication of hydrocarbon bands in the spectrum of the Nova.

"Up to the present eleven mostly very good spectrographic photographs have been taken; they were obtained by means of a small spectrograph connected to the photographic refractor of 34 cm. aperture. The dispersion is only small, but in the small spectrum of 10 mm. length, extending from F to H, much detail is discernible. The illuminating power of the apparatus is so great, in spite of the narrow slit employed, that even now an exposure of 40 minutes is sufficient to obtain an image suitable for measurement. The bright hydrogen lines F, Hγ, *h*, H, and the calcium line H_2, are very broad; and, as already announced, the corresponding dark lines of a second spectrum are displaced with respect to the bright lines towards the violet, and in spite of the breadth of the latter, are almost entirely separated. There are still some of the hydrogen lines in the ultra-violet visible, but they are too faint for any approximately certain observation.

"In the last few days the spectrum has changed, inasmuch as in the broad bright lines Hγ, *h*, H, and H_2 (F is only traced on plates which are over-exposed for the middle of the photographic spectrum), two maxima of intensity are plainly discernible, and, as in each of the corresponding dark lines, a narrow bright line has appeared. From the measurements, a connection between these and the hydrogen lines appears beyond doubt, and it is

not improbable that these linear brightenings in the broad dark lines indicate eruptions of gases from the interior of the body possessing the continuous spectrum with the dark absorption lines. Such brightenings are occasionally seen in the spectra of sun-spots. On this supposition, the fine bright lines would indicate very nearly the middle of the dark lines.

"The appearance of two maxima of intensity in the broad bright lines admits of the conclusion that two bodies with different motions possess spectra with bright lines, and that therefore the spectrum of the Nova consists of at least three spectra superposed, from the measurement of which, in connection with the comparison spectra of β Aurigæ or β Tauri on the same plate, the relative motions of the three supposed bodies, as well as their motions with respect to the earth, can be determined. Denoting the body with the dark-line spectrum by a, the two others with bright-line spectra by b and c, measurements by myself and Dr. Scheiner have given the following results:

$$a - \tfrac{1}{2}(b + c) = 120 \text{ miles},[1]$$
$$b - c = 70 \text{ miles}.$$

and further with respect to the earth—

$$a = -90 \text{ miles}, \; b = -5, \; c = +65 \text{ miles}.$$

"This result is still very uncertain, and must be regarded as quite preliminary, for it is evident that with the small size of the spectra the accuracy cannot be pushed very far—a displacement of ·01 mm. corresponds, for instance, to a motion of 8 to 12 miles, according to the situation of the line in the spectrum—and that the size of the silver grain in the photographs can exert a very marked influence on the measurements.

"In the photographic spectrum of the Nova, besides the broad lines mentioned, several more bright and mostly very broad lines can be seen, whose wave-lengths I intend to communicate later on."

Prof. Pickering communicates some valuable information to the same number of the *Astronomische Nachrichten* with reference to the visibility of the Nova before its discovery by Dr. Anderson. In eighteen photographs of this region, which were taken by the 8-inch photographic telescopes between the dates November 3, 1885, and November 2, 1891, no star in the Nova's place was visible, but in those taken from December 16, 1891, to January 31, 1891, there was a star of the fifth magnitude recorded. In another series of plates taken with the transit photometer, no record of the new star up to December 1, 1891, was obtained, although χ Aurigæ (mag. 5·0 m.) was always visible, but the plates taken on the nights of December 10, 1891, and ending January 20, 1892, indicated clearly the position of the new star.

Careful examination has been made on all the above-mentioned plates, and the following extract shows the series of magnitudes which have been deduced from the measurements:—

"It appears that the star was fainter than the eleventh magnitude on November 2, 1891, than the sixth magnitude on December 1, and that it was increasing rapidly on December 10. A graphical construction indicates that it had probably attained the seventh magnitude within a day or two of December 2, and the sixth magnitude December 7. The brightness increased rapidly until December 18, attaining its maximum about December 20, when its magnitude was 4·4m. It then began to decrease slowly, with slight fluctuations, until January 20, when it was slightly below the fifth magnitude."

From this it will be seen that two months' observations have been lost, owing to its late discovery.

ABERRATION.[2]

UNDER this head may conveniently be considered not only the apparent displacement of the stars discovered by Bradley, but other kindred phenomena dependent upon the velocity of light bearing but a finite ratio to that of the earth in its orbit round the sun, and to other astronomical velocities.

The explanation of stellar aberration, as usually given, proceeds rather upon the basis of the corpuscular than of the wave theory. In order to adapt it to the principles of the latter theory, Fresnel found it necessary to follow Young in assuming that the æther in any vacuous space connected with the earth (and therefore practically in the atmosphere) is undisturbed by the earth's motion of 19 miles per second. Consider for simplicity the case in which the direction of the star is at right angles to that of the earth's motion, and replace the telescope, which would be used in practice, by a pair of perforated screens, on which the light falls perpendicularly. We may further imagine the luminous disturbance to consist of a single plane pulse. When this reaches the anterior screen, so much of it as coincides with the momentary position of the aperture is transmitted, and the remainder is stopped. The part transmitted proceeds upon its course through the æther independently of the motion of the screens. In order, therefore, that the pulse may be transmitted by the aperture in the posterior screen, it is evident that the line joining the centres of the apertures must not be perpendicular to the screens and to the wave front, as would have been necessary in the case of rest. For in consequence of the motion of the posterior screen in its own plane the aperture will be carried forward during the time of passage of the light. By the amount of this motion the second aperture must be drawn backwards, in order that it may be in the place required when the light reaches it. If the velocity of light be V, and that of the earth be v, the line of apertures, giving the apparent direction of the star, must be directed forwards through an angle equal to v/V. More generally, if the angle between the star and the point of the heavens towards which the earth is moving be α, there will be an apparent displacement towards the latter point, expressed by $\sin \alpha \, . \, v/V$, and independent of the position upon the earth's surface where the observation is made. The ratio v/V is about $\frac{1}{10000}$.

The aperture in the anterior screen corresponds to the object-glass of the telescope with which the observation would actually be made, and which is necessary in order to produce agreement of phase of the various elementary waves at a moderately distant focal point. The introduction of a refracting medium would complicate the problem, and is not really necessary for our present purpose. As has been shown (*Philosophical Magazine*, March 1881, "On Images formed without Reflection or Refraction"), the only use of an object-glass is to shorten the focal length. Our imaginary screens may be as far apart as we please, and if the distance is sufficient, the definition, and consequently the accuracy of alignment, is as great as could be attained with the most perfect telescope whose aperture is equal to that in the anterior screen.

It appears, then, that stellar aberration in itself need present no particular difficulty on the wave theory, unless the hypothesis of a quiescent æther at the earth's surface be regarded as such. But there are a variety of allied phenomena, mostly of a negative kind, which require consideration before any judgment can be formed as to the degree of success with which the wave theory meets the demands made upon it. In the first place, the question arises whether terrestrial optical phenomena could remain unaffected by the supposed immense relative motion of our instruments and of the æther; whether reflection, diffraction, and refraction, as ordinarily observed by us, could be independent of the direction of the rays relatively to the earth's motion. It may be stated at once that no such influence has been detected, even in experiments carefully designed with this object in view.

Another class of experiments, with the results of which theory must be harmonized, are those of Fizeau and Michelson upon the velocity of light in ponderable refracting media which have a rapid motion (relatively to the instruments and other surrounding bodies) in the direction of propagation, or in the opposite direction. These very important researches have proved that in the case of water the velocity of the ponderable medium is not without effect; but that the increment or decrement of the velocity of propagation is very decidedly less than the velocity of the water. On the other hand, the motion of air, even at high velocities, has no perceptible influence upon the propagation of light through it.

Again, it has been found by Airy,[1] as the result of an experiment originally suggested by Boscovitch, that the constant of stellar aberration is the same, whether determined by means of a telescope of the ordinary kind, or by one of which the tube is filled with water. It is clear that, according to Fresnel's views

[1] = about 540 English miles.—*Tr.*

[2] This paper was written in 1887, when I was occupied with my article upon "Wave Theory" for the "Encyclopædia Britannica," and at a time when a more extensive treatment was contemplated than was afterwards found practicable. Friends on whom I can rely are of opinion that its publication may be useful; and, as I am not able to give it a complete revision, I prefer to let it stand under its original date, merely warning the reader that very important work has since been published by Michelson.—*January* 1892.

[1] Proc. Roy. Soc., xx., 1872, p. 35; xxi., 1873, p. 121.

of the condition of the æther at the earth's surface, this agreement must involve some particular supposition as to the propagation of light in moving refracting media.

The theory of these phenomena must evidently turn upon the question whether the æther at the earth's surface is at rest, absolutely, or relatively to the earth;[1] and this fundamental question has not yet received a certain answer. The independence of terrestrial optical phenomena of the earth's motion in its orbit is, of course, more easily explained upon the latter alternative; or rather no explanation is required. But in that case the difficulty is thrown upon stellar aberration, which follows a more simple law than we should expect to apply in the case of an æther disturbed by the passage of a body in its neighbourhood. Prof. Stokes has, indeed, attempted a theory on these lines,[2] by supposing the ætherial motion to be what is called in hydrodynamics irrotational. In strictness there is, however, no such motion possible, subject to the condition of vanishing absolutely at a great distance, and relatively at the earth's surface; and it does not appear that the objection thus arising can be satisfactorily met.

If we start from the experimental facts which have the most direct bearing upon the question under discussion, we are led to regard Fresnel's views (doubtless in some generalized form) as the more plausible. From the results of Fizeau and Michelson relative to air, we may conclude with tolerable confidence that a small mass of ponderable matter, of very low refracting power, moving in space, would not appreciably carry the æther with it. The extension of the argument to a body as large as the earth is not unnatural, though it involves certainly an element of hypothesis. In like manner, if the globe were of water, we should expect the æther to be carried forward, but not to the full amount. The simple t supposition open to us is that, in any kind of ponderable matter, forming part of a complex mass, the æther is carried forward with a velocity dependent upon the local refracting power, but independent of the refracting power and velocity of other parts of the mass. In the earth's atmosphere, where the refracting power is negligible, the æther would be sensibly undisturbed.

If we agree to adopt this point of view provisionally, we have next to consider the relation between the velocity of luminous propagation in moving ponderable matter and the refractive index. The character of this relation was discovered by Fresnel, whose argument may be thrown into the following form.

Consider the behaviour of the æther when a plate of ponderable matter (index = μ) is carried forward through vacuum with velocity v in a direction perpendicular to its plane. If D be the density of the æther in vacuum, and D_1 the density in the refracting medium, then, according to Fresnel's views as to the cause of refraction, $D_1 = \mu^2 D$. The æther is thus condensed as the plate reaches it; and if we assume that the whole quantity of æther is invariable, this consideration leads to the law giving the velocity (xv) with which the denser æther within the plate must be supposed to be carried forward. For conceive two ideal planes, one in the plate and one in the anterior vacuous region, to move forward with velocity v. The whole amount of æther between the planes must remain unchanged. Now, the quantity entering (per unit area and time) is Dv, and the quantity leaving is $D_1(v - xv)$. Hence,

$$x = 1 - \mu^{-2},$$

so that the velocity with which the æther in the plate is carried forward is $v(1 - \mu^{-2})$, tending to vanish as μ approaches unity. If V be the velocity of light in vacuum, and V/μ the velocity in the medium at rest, then the absolute velocity of light in the moving medium is

$$V/\mu \pm v(1 - \mu^{-2}). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Whatever may be thought of the means by which it is obtained, it is not a little remarkable that this formula, and no other, is consistent with the facts of terrestrial refraction, if we once admit that the æther in the atmosphere is at absolute rest. It is not probable that the æther, in moving refracting bodies, can properly be regarded as itself in motion; but if we knew more about the matter we might come to see that the objection is verbal rather than real. Perhaps the following illustration may assist the imagination. Compare the æther in vacuum to a stretched string, the transverse vibrations of which represent light. If the string is loaded, the velocity of propagation of waves is diminished. This represents the passage of light through stationary refracting matter. If now the loads be imagined to run along the string with a velocity not insensible in comparison with that of waves, the velocity of the latter is modified. The substitution of a membrane for a string will allow of a still closer parallel. It appears that the suggested model would lead to a somewhat different law of velocity from that of Fresnel; but in bringing it forward the object is merely to show that we need not interpret Fresnel's language too literally.

We will now consider a few examples of the application of the law of velocity in a moving medium; and first to the experiment of Boscovitch, in which stellar aberration is observed with a telescope filled with water. We have only to suppose the space between the two screens of our former explanation to be occupied by water, which is at rest relatively to the screens. In consequence of the movement of the water, the wave, after traversing the first aperture, is carried laterally with the velocity $v(1 - \mu^{-2})$, and this is to be subtracted from the actual velocity v of the aperture in the posterior screen. The difference is $\mu^{-2}v$. The ratio of this to the velocity of light in water (V/μ) gives the angular displacement of the second aperture necessary to compensate for the motion. We thus obtain $\mu^{-1}v/V$. This angle, being measured in water, corresponds to v/V in air; so that the result of the motion is to make the star appear as if it were in advance of its real place by the angle v/V, precisely as would have happened had the telescope contained air or vacuum instead of water.

We will now calculate the effect of the motion of a plate perpendicular to its own plane upon the retardation of luminous waves moving in the same (or in the opposite) direction. The velocity of the plate is v, its index is μ, and its thickness is d. Denoting, as before, the velocity of the æther within the plate by xv, and supposing, in the first place, that the signs of v and V are the same, we have, for the absolute velocity of the wave in the plate,

$$V/\mu + xv.$$

We have now to express the time (t) occupied by the wave in traversing the plate. This is not to be found by simply dividing d by the above written velocity; for during the time t the anterior face of the plate (which the wave reaches last) is carried forward through the distance vt. Thus, to determine t we have

$$(V/\mu + xv)t = d + vt,$$

whence

$$\frac{Vt}{d} = \frac{\mu}{1 + (x - 1)\mu v/V}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The time, t_0, which would have been occupied in traversing the same distance ($d + vt$), had the plate been away, is given by

$$Vt_0 = d + vt;$$

so that

$$\frac{Vt_0}{d} = 1 + \frac{\mu v/V}{1 + (x - 1)\mu v/V}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Thus

$$\frac{V(t - t_0)}{d} = \frac{\mu(1 - v/V)}{1 + (x - 1)\mu v/V} - 1. \quad . \quad . \quad . \quad . \quad (4)$$

Substituting in this Fresnel's value of x, viz. $(1 - \mu^{-2})$, and neglecting as insensible the square of v/V, we find

$$V(t - t_0) = (\mu - 1)d(1 - v/V). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

If we suppose that part of the original wave traverses the plate, and that part passes alongside, (5) gives the relative retardation —that is, the distance between the wave fronts which were originally in one plane. It would appear at first sight that this result would give us the means of rendering v evident. For the retardation, depending upon the sign of v/V, will be altered when the direction of the light is reversed, and this we have it in our power to bring about by simply turning our apparatus through 180°. A more careful examination will, however, lead us to a different conclusion.

The most obvious way of examining the retardation would be to use homogeneous light, and, by producing regular interference of the two portions, to observe the position of the fringes, and any displacement that might result from a shift of the apparatus relatively to the direction of the earth's motion. But if we employ for this purpose a terrestrial flame, *e.g.* that of a Bunsen's burner containing sodium, we have to take into

[1] An accusation of crudeness might fairly be brought against this phraseology; but an attempt to express the argument in more general language would probably fail, and would in any case be tedious.

[2] *Phil. Mag.*, xxviii., 1846, p. 76; xxix., 1846, p. 6.

account the fact that the source is itself in motion. For it is evident that the waves which pass in a given time through any point towards which the source is moving are more numerous than had the source been at rest, and that the wave-lengths are correspondingly shortened. If v be the velocity of the source, the wave-length is changed from λ to $\lambda(1 - v/V)$. At a point behind, from which the source is retreating, the wave-length is $\lambda(1 + v/V)$. We shall have occasion to refer again to this principle, named after Döppler, as applied by Huggins and others to the investigation of the motion of the heavenly bodies in the line of sight.

Referring now to (5), we see that, although the *absolute* retardation is affected by v, yet that the retardation *as measured in wave-lengths* remains unaffected. If, then, there be, in the absence of v, an agreement of phase between the two interfering beams, the introduction of v will cause no disturbance. Consequently no shifting of the interference bands is to be expected when the apparatus is turned so that the direction of propagation makes in succession all possible angles with that of the earth's motion.

The experiment has been modified by Hoek,[1] who so arranged matters as to eliminate the part of the retardation independent of v. As before, of two parallel beams A and B, one, A, passes through a plate of refracting medium; the other, B, through air. The beams are then collected by a lens, at the principal focus of which is placed a mirror. After reflection by this mirror, the beams exchange paths, B returning through the plate, and A through air. Apart, therefore, from a possible effect of the motion, there would be complete compensation and no final difference of path. As to the effect of the motion, it would appear at first sight that it ought to be sensible. During the first passage, A is (on account of v) accelerated; on the return, B is retarded; and thus we might expect, upon the whole, a relative acceleration of A equal to $(\mu - 1)d \,.\, 2v/V$. But here, again, we have to consider the fact that another part of the apparatus, viz. the mirror, partakes in the motion. In the act of reflection the original retardation of A is increased by twice the distance through which the mirror retreats in the interval between the arrival of the two waves. This distance is (with sufficient approximation) $(\mu - 1)d \,.\, v/V$; so that the influence of the movement of the mirror just compensates the acceleration of A which would have resulted in the case of a fixed mirror. On the whole, then, and so long as the square of v/V may be neglected, no displacement of fringes is to be expected when the apparatus is turned. The fact that no displacement was observed by Hoek, nor in an analogous experiment by Mascart,[2] proves that if the stationary condition of the æther in terrestrial vacuous spaces be admitted, we are driven to accept Fresnel's law of the rate of propagation in moving refracting media.

What is virtually another form of the same experiment was tried by Maxwell,[3] with like negative results. In this case, prisms were used instead of plates; and the effect if existent, would have shown itself by a displacement of the image of a a spider-line when the instrument was turned into various azimuths.

On the basis of Fresnel's views it may, in fact, be proved generally that, so far as the first power of v/V is concerned, the earth's motion would not reveal itself in any phenomenon of terrestrial refraction, diffraction, or ordinary refraction. The more important special cases were examined by Fresnel himself, and the demonstration has been completed by Stokes.[4] Space will not allow of the reproduction of these investigations here, and this is the less necessary, as the experiment of Hoek, already examined, seems to raise the principal question at issue in the most direct manner.

Another point remains to be touched upon. We have hitherto neglected *dispersion*, treating μ as constant. In stationary dispersing media, μ may be regarded indifferently as a function of the wave-length or of the periodic time. When, however, the medium is in motion, the distinction acquires significance; and the question arises, What value of μ are we to understand in the principal term V/μ of (1)? Mascart points out that the entirely negative results of such experiments as those above described indicates that, in spite of the difference of wave-length due to the motion, we must take the same value of μ as if the medium and the source had been at rest, or that μ is to be regarded as a function of the *period*.

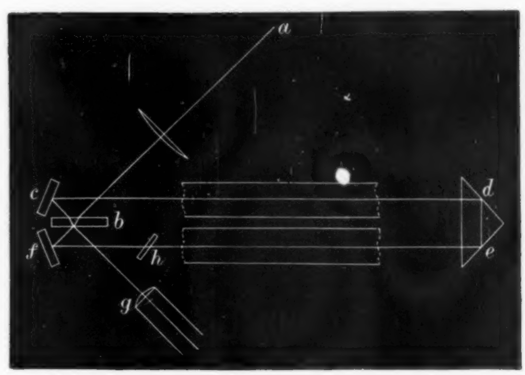
[1] *Archives Néerlandaises*, t. iii. p. 180 (1868); t. iv. p. 443 (1869).
[2] *Ann. de l'École Normale*, t. iii. (1874).
[3] Phil. Trans., 1868, p. 532.
[4] *Phil. Mag.*, xxviii. p. 76 (1846). See also Mascart, *Ann. de l'École Norm.*, t. i. (1872), t. iii. (1874); and Verdet, "Œuvres," t. iv., deuxième partie.

Mascart has experimented also upon the influence of the earth's motion upon double refraction, with results which are entirely negative. The theoretical interpretation must remain somewhat ambiguous, so long as we remain in ignorance of the mechanical cause of double refraction.

Reference has already been made to the important experiments by Fizeau and by Michelson upon the velocity of light in moving media. The method, in its main features, is due to the former,[1] and is very ingeniously contrived for its purpose. Light issuing from a slit is rendered parallel by a collimating lens, and is then divided into two portions, which traverse tubes containing running water. After passing the tubes, the light falls upon a focussing lens and mirror (as in Hoek's experiment), the effect of which is to interchange the paths. Both rays traverse both tubes; and, consequently, when ultimately brought together, they are in a condition to produce interference bands. If now the water is allowed to flow through the tubes in opposite directions, one ray propagates itself throughout *with* the motion of the water, and the other *against* the motion of the water; and thus, if the motion has any effect upon the velocity of light, a shift of the bands is to be expected. This shift may be doubled by reversing the flow of water in the tubes.

Fizeau's investigation has recently been repeated in an improved form by Michelson.[2]

"Light from a source at a falls on a half-silvered surface, b, where it divides; one part following the path $b\,c\,d\,e\,f\,b\,g$, and the other the path $b\,f\,e\,d\,c\,b\,g$. This arrangement has



the following advantages: (1) it permits the use of an extended source of light, as a gas flame; (2) it allows any distance between the tubes which may be desired; (3) it was tried by a preliminary experiment, by placing an inclined plate of glass at h. The only effect was either to alter the width of the fringes, or to alter their inclination; *but in no case was the centre of the central white fringe affected*. Even holding a lighted match in the path had no effect on this point.

"The tubes containing the fluid were of brass, 28 mm. internal diameter; and in the first series of experiments, a little over 3 metres in length, and in the second series a little more than 6 metres."

Even with the longer tubes and the full velocity (about 8 metres per second) the displacement on reversal amounted to less than the width of a fringe. Nevertheless, fairly concordant results were arrived at; and they showed that the fraction (x) of the velocity of the water (v) by which the velocity of light is altered is ·434, with a possible error of ± ·02. The numerical value of the theoretical expression is

$$x = 1 - \mu^{-2} = {\cdot}437,$$

in very close accordance.

"The experiment was also tried with air moving with a velocity of 25 metres per second. The displacement was about ·01 of a fringe; a quantity smaller than the probable error of

[1] *Ann. de Chimie*, III. lvii. (1859).
[2] *American Journal*, vol. xxxi. p. 377 (1886).

observation. The value calculated from $(1 - \mu^{-2})$ would be ·0036."

We have seen that, so far as the first power of v/V is concerned, Fresnel's theory agrees with all the facts of the case. The question whether it is possible to contrive an experiment in which v^2/V^2 shall be sensible, has been considered by Michelson,[1] who, having arrived at an affirmative conclusion, proceeded to attack this very difficult experimental problem. In Michelson's apparatus interference is brought about between two rays, coming of course originally from the same source, one of which has traversed to and fro a distance, D, parallel to the earth's motion, and the other a like distance in the perpendicular direction. The phase of the latter ray is considered by Michelson to be unaffected by the earth's motion. As to the former, it is retarded by the amount

$$\frac{D}{V - v} + \frac{D}{V + v} - \frac{2D}{V} = \frac{2D}{V} \cdot \frac{v^2}{V^2},$$

or, reckoned in distance at velocity V,

$$2D\,v^2/V^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

"Considering only the velocity of the earth in its orbit, the ratio $v/V = 10^{-4}$ approximately, and $v^2/V^2 = 10^{-8}$. If $D = 1200$ mm., or, in wave-lengths of yellow light, 2,000,000, then in terms of the same unit, $2D\,v^2/V^2 = {·}04$.

"If, therefore, an apparatus is so constructed as to permit two pencils of light, which have travelled over paths at right angles to each other to interfere, the pencil which has travelled in the direction of the earth's motion, will in reality travel ·04 of a wave-length further than it would have done were the earth at rest. The other pencil, being at right angles to the motion, would not be affected.

"If now the apparatus be revolved through 90°, so that the second pencil is brought into the direction of the earth's motion, its path will be lengthened ·04 wave-length. The total change in the position of the interference bands would be ·08 of the distance between the bands, a quantity easily measurable."

In the actual experiment, the earth's velocity was not available to the full extent, and the displacement to be expected on this account was reduced to ·048; but Michelson considers that some addition to it should be made on account of the motion of the solar system as a whole. The displacement actually found was ·022; and when the apparatus was employed in such azimuths that the rotation should have had no effect in any case, ·034. These results are very small, and Michelson gives reasons for regarding them as partially systematic errors of experiment. He concludes that there is no real displacement of the bands, and that the hypothesis of a stationary æther is thus shown to be inconsistent with fact.

It has, however, been recently pointed out by Lorentz[2] that Michelson has over-estimated the effect to be expected according to Fresnel's views. The ray which travels perpendicularly to the earth's motion is not unaffected thereby, but is retarded to the amount represented by $D\,v^2/V^2$. The outstanding relative retardation is thus only $D\,v^2/V^2$, instead of the double of that quantity. Accepting this correction, we have to expect, according to Fresnel's views, a shift of only ·024 of a band in Michelson's experiment.

Under these circumstances Michelson's results can hardly be regarded as weighing heavily in the scale. It is much to be wished that the experiment should be repeated with such improvements as experience suggests. In observations spread over a year, the effects, if any, due to the earth's motion in its orbit, and to that of the solar system through space, would be separated.

On the whole, Fresnel's hypothesis of a stationary æther appears to be at the present time the more probable; but the question must be considered to be an open one. Further evidence would be most important; but it is difficult to see from what quarter anything essentially new can be expected. It might be worth while for astronomers to inquire whether it is really true, as is generally assumed, that stellar aberration is independent of the position upon the earth's surface from which the observation is made. Another question that might, perhaps, be submitted with advantage to an experimental examination is whether the propagation of light in air is affected by the rapid motion of heavy masses parallel to, and in the immediate neighbourhood of, the ray.

If we once admit the principle that, whatever the explanation may be, no ordinary[1] terrestrial observation is affected by the earth's motion, it is easy to give an account of what must happen when the light comes from an external source which may have a motion in the line of sight. Imagine, for example, a spectroscopic examination of a soda flame situated on a star and vibrating in identical periods with those of terrestrial soda flames. In accordance with Döppler's principle, the wave-lengths are altered by a relative motion in the line of sight, and the fact may be rendered evident by a comparison between the spectra of the star and of the terrestrial flame, held so as to be seen in the same direction. The simplest case is when the flame is entirely external to the apparatus, so that both lights are treated in precisely the same way. It is evident that, under these circumstances, the difference between the two cannot fail to become apparent; and this way of regarding the matter shows also that the apparent displacement of the bright lines in the stellar spectrum is dependent upon the relative, and not further upon the absolute, motions of the star and of the earth. The mean of observations, equally distributed over the year, would thus give data for determining the relative motion in the line of sight of the star and of the solar system.

If the external source be the sun itself, it might be thought that the spectra must agree almost perfectly, the eccentricity of the earth's orbit being so very small. But the sun is a revolving body, and consequently a distinction must be made according to the part of the sun from which the light proceeds. It is found, in fact, that a very sensible shift takes place in the position of the dark lines according as the light under observation comes from the advancing or from the retreating limb. This circumstance has been successfully employed by Thollon and Cornu to distinguish between lines having a solar and a terrestrial origin. In the latter case it is a matter of indifference from which part of the sun the light proceeds.

In general optical theory the finiteness of the velocity of light is usually disregarded. Velocities at least ten times greater than that of the earth in its orbit are, however, known to astronomers; and such must begin to exercise a sensible influence upon radiation. Moreover, in so wide a generalization as the theory of exchanges, the neglect of even a small quantity is unsatisfactory. Prof. Balfour Stewart[2] has discussed the influence of the motion of a plate exercising selective absorption upon the equilibrium of radiation within an inclosure. He argues that a disturbance will ensue, involving a violation of the second law of thermodynamics, unless compensated by some other effect not hitherto recognized. It appears, however, more probable that the whole radiation coming *from* and *through* a plate would not be altered by its motion. Whatever effect (in accordance with Döppler's law) the motion has upon the radiation from the plate, a similar effect would probably be produced upon the absorbing power. On this view the only result of the motion would be to change the wave-length of the rays most powerfully emitted and absorbed, but without disturbing the balance required by the theory of exchanges. The moving plate would in fact be equivalent to a stationary one of slightly different quality. RAYLEIGH.

1887.

[1] *American Journal*, xxii. p. 120 (1881).

[2] "Over den invloed dien de beweging der aarde of de licht verschijnselen uitoefent." (Amsterdam, 1886.)

[1] This qualification is inserted in order to exclude such an experiment as that of Michelson, just described, in which an attempt is made to render sensible an effect depending on v^2/V^2.

[2] B.A. Report, 1871.

SOCIETIES AND ACADEMIES.

LONDON.

Mathematical Society, March 10.—Prof. Greenhill, F.R.S., President, in the chair.—The President and Mr. S. Roberts, F.R.S., spoke upon the loss the Society had sustained by the recent decease of Dr. Hirst, F.R.S., touching more especially upon the great services he had rendered to it in the early days of its existence.—The following paper was read:—The simplest equivalent of a given optical path, and the observations required to determine it, by Dr. J. Larmor. To specify an optical path through a heterogeneous medium like the atmosphere, or through an arrangement of refracting substances like an optical instrument, we require the geometrical curve followed by the filament of light, and also the character of the modification produced on a filament following this path across the medium.

This division into filaments of waves, along whose course the energy of the radiation is propagated, is the true objective analysis of the light; and it also, on Sir W. R. Hamilton's principles, leads to more compact and comprehensive treatment than the ordinary analysis into linear rays. It is shown that the effect of the medium on any filament following the given path is exactly equivalent to that of a certain pair of thin astigmatic lenses with a common axis, on a beam passing across them; and a method is given for constructing these lenses from observations made at the two extremities of the actual optical path. It is shown, in continuation, that conjugate pairs of focal lines at the two ends of a filament are given by a 2 to 2 correspondence, whose general relations are exhibited graphically by the aid of a pair of conics, and various developments are made in this direction. There are, in general, no points which have conjugate foci; but, if a certain condition hold, there exist two transverse planes of points which have conjugate planes of focal points. This occurs only when the equivalent astigmatic pair of lenses have their principal sections parallel, so that the component refractions in these principal sections are independent of each other. It is only in this special case that the emergent filament can be constructed by means of rays, in Moebius's manner, by aid of the two conjugate pairs of focal planes. If a certain other condition is also satisfied, there is complete symmetry round the axis, except as regards a possible constant rotation of the filament; and then the optical path is equivalent to refraction by a single ordinary thin lens.—The President communicated a paper, by Prof. W. Burnside, on cases in which a hyper-elliptic integral of the first order can be expressed as the sum of two elliptic integrals.—Mr. Tucker read abstracts of the following notes:—On the analytical theory of the congruency, by Prof. Cayley, F.R.S.—On certain curves of the fourth order, and the porism of the inscribed and circumscribed polygon, by Mr. R. A. Roberts.—Notes on dualistic differential transformations, by Mr. E. B. Elliott, F.R.S. A perusal some time ago of De Morgan's paper in the Cambridge Transactions on the subject of the principle of duality in differential equations which bears his name led Mr. Elliott to notice a short note thereto, in which the author announced that after writing his paper he had found a note by Chasles, in which the method had been anticipated. Upon this, referring to Chasles's work ("Aperçu Historique," note xxx), he found that Chasles had stated and to a certain extent developed a theory on the subject of much wider generality. It occurred to Mr. Elliott that some further consideration might with advantage be given to Chasles's conclusions and their extension; and a portion of this paper is the result. It had previously occurred to the author that recent theories as to the transformation of differential expressions by interchange of dependent and independent variables, and in particular the theory of reciprocants, had a bearing on the more restricted or De Morgan duality, and even more on its simpler analogue as to ordinary differential equations, which had probably escaped notice. Another portion of these notes is devoted to the elucidation of this idea.—Prof. M. Hill made a few remarks on singular solutions; and the President spoke on the rectification of the Cartesian oval. It has been shown by Prof. Genocchi, of Turin (*Annali di Matematica*, 1864), and by Mr. Samuel Roberts (Proc. L.M.S. iii.), that the arc of a Cartesian oval can be expressed as the sum of three elliptic arcs. Taking a fixed oval (i.) and its conjugate oval (ii.) in a triconfocal system of Cartesians, then as a variable orthogonal oval traces out by its intersection with (i.) a certain arc, its conjugate oval traces out on (ii.) another arc; the sum of these arcs can be expressed by a single elliptic arc, while the difference is expressible as the sum of two elliptic arcs; thus leading to the theorems of Prof. Genocchi and Mr. S. Roberts.

Anthropological Institute, March 8.—Edward B. Tylor, F.R.S., President, in the chair.—Mr. J. Allen Brown read a paper on the continuity of the Palæolithic and Neolithic periods. The deductions of the author are based on the large number of flint implements of Palæolithic type which have been discovered during recent years at Eastbourne, East Dean, Cuckmere, and in other combes and dry valleys in England; at East Dean, &c., they are associated with compact aggregated deposits of flints and chalk rubble, evidently due to the erosion of the valleys and combes by underground water, as seen at Birling Gap, near Eastbourne. The valleys of Sussex have been subject to many changes during the concluding episodes (both glacial and subaerial) of the Quaternary period, and in many cases the older forms of flint implements have been covered up and preserved by the deposit of loam and chalk rubble resulting from the waste of the surface of the land. Intermixed or associated with the flint implements of older types are others of transition form, to which he desired to see the term "Mesolithic" applied. The East Dean Valley appeared to contain flint implements forming a series ranging from the late Palæolithic age to the polished stone period of true Neolithic. The old mining-shaft at Cissbury has furnished analogous specimens. Similar implements of the Palæolithic type have been found in chalk rubble far away from the sea-board, and associated with the bones of the mammoth, tichorine rhinoceros, hippopotamus, and other Quaternary Mammalia, as well as the remains of various animals of species still living, showing that man was present in Southern Britain not only in the plateaux and river-drift periods, but also continuously into the so-called Neolithic epoch. The author alluded to the evidence derived from caves and rock-shelters and peat-beds, both in this country and in France, which pointed in the same direction. A large series of flint implements of Palæolithic form from East Dean, &c., were exhibited, with specimens of corresponding forms from the river-drift; also a series showing the evolution of the axe or celt form from the simply chipped nodule of the plateaux drift, through the valley drift and transitional types to the highly finished celts of the Neolithic age, of which the forms were continued in the earliest stages of the age of copper and bronze. Other series were exhibited, showing in like manner the evolution of the spear-head and knife, &c.

Zoological Society, March 1.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1892, and called attention to two Short-winged Tyrants (*Machetornis rixosa*) purchased February 15, being the first examples of this bird that have reached the Society, and to a female Beatrix Antelope (*Oryx beatrix*) from Arabia, presented by Lieut.-Colonel Talbot, February 18.—Mr. J. Graham Kerr gave a short account of the expedition up the Rio Pilcomayo in 1890–91, which he had accompanied as naturalist. Mr. Kerr made remarks on the animals met with on the banks of the Pilcomayo, and exhibited a series of photographs illustrating the vegetation of the district and its native Indian inhabitants.—Mr. G. F. Hampson read a paper on stridulation in certain Lepidoptera, and on the distortion of the hind-wings in the males of certain *Ommatophorinæ*. The author attributed the clicking sound described by Darwin as produced by various species of the South American genus of Butterflies, *Angerona*, and confirmed by Wallace and other observers, to the presence of a pair of strong corneous hooks on the thorax, which play on a pair of curved hooks with spatulate ends attached to the inner margin of the fore-wing close to the base, and surrounded by a membranous sac which acts as a sounding-board. An account was given of a similar sound produced by the males of a Burmese moth of the family Agaristidæ and of a buzzing sound in an allied Australian form, both of which have a patch of ribbed hyaline membrane below the costa of the fore-wing. The sound was attributed to the friction of spines, attached in the former to the first pair of legs, in the latter to the second pair, on the ribbed membrane. A description was then given of the transformation of the costal half of the hind-wing in the Noctuid genus *Patula* into a large scent-gland, and of the manner in which this had distorted the neuration. The still greater distortion of the neuration in the allied genus *Argida* was attributed to its once having possessed a similar scent-gland, now become rudimentary by disuse.—A communication was read from Prof. W. N. Parker, on the retention of functional gills in young Frogs (*Rana temporaria*), which he had succeeded in producing in specimens reared in his laboratory. Prof. Parker described the method employed with this object, and made remarks on the way in which the fore-limbs are protruded.—Prof. F. Jeffrey Bell read a paper entitled "A Contribution to the Classification of Ophiuroids," to which were added descriptions of some new and little-known forms of this group.—Mr. M. F. Woodward gave an account of an abnormal Earthworm (*Lumbricus terrestris*) possessing seven pairs of ovaries situated on the eighth and following somites to the fourteenth.

OXFORD.

University Junior Scientific Club, March 4.—Mr. J. A. Gardner, of Magdalen College, President, in the chair.—Some investigations of the action of dry hydrochloric acid gas on dry carbonates were brought forward by Mr. F. R. L. Wilson,

Keble College, and Mr. R. E. Hughes, Jesus College. The authors showed that probably perfectly dry hydrochloric acid gas does not act on carbonates. Experiments were tried with the carbonates of calcium and barium.—Mr. J. L. Hatton, Hertford College, read a paper on some investigations, which he had been engaged upon, in conjunction with Mr. James Walker, on the motions of the nodal planes in a rotating bell. This work appeared in a recent number of the *Philosophical Magazine*.—This paper was followed by an account of the fixation of nitrogen by plants, by Mr. O. V. Darbishire, of Balliol College.

Paris.

Academy of Sciences, March 14.—M. d'Abbadie in the chair.—The Secretary commented upon the loss sustained by the Academy by the death of M. Léon Lalanne.—On conical vascular branches, and on the inductions to which they lead with regard to the organization of the vascular blood system, by M. Ranvier.—Researches on samarium, by M. Lecoq de Boisbaudran. By passing an electric spark from a large induction coil, without condensers, through solutions rich in samarium, and viewing the spark spectroscopically, lines were obtained at the wave-lengths 466·2, 462·7, and 459·3, and a wide band having a well-defined edge at λ 611·2, and fading away to about λ 622. The samarium bands undergo very marked variations when the position of the spark with respect to the meniscus of liquid is altered. This fact is thought to be of interest from the point of view of the supposed complexity of samarium. It is not impossible that there is a relation between the band 611–622 and the narrow line which Prof. Crookes observed when using mixtures of samarium and yttrium *in vacuo*, and which he attributed to the presence of a new element. M. Boisbaudran has observed this line, or one very near it, with different substances, and finds that its position varies sensibly with the nature of the solution employed. The narrow line is accompanied with a less refrangible and weaker one. With lanthanum sulphate mixed with a compound of samarium, the wave-length of the stronger line was determined as 612·7, and of the weaker 619·6. Prof. Crookes obtained the wave-length 609.—On a remarkable prominence, by M. H. Deslandres. The prominence was observed on the east limb of the sun on March 3, as the large spot-group of February was coming round it.—On frictionless gearings, by M. A. Rateau.—On periodic heat maxima observed in spectra furnished by flint and crown glass, and rock-salt, by M. Aymonnet. The heat maxima observed are separated by equal wave-lengths in the case of each of the prisms used, and, for rock-salt, the maxima appear to correspond to the fundamental vibrations of 1, 2, 3 . . . n systems of cubical molecules.—On some well-defined alloys of sodium, by M. Joannis. By the action of lead, in excess, upon sodammonium, a compound having the formula $Pb_2Na,2NH_3$ was obtained. An alloy of lead and potassium, Pb_2K, was obtained by the action of potassammonium, in excess, upon lead; an alloy of bismuth and sodium, $BiNa_3$, by treating pure bismuth, in excess, with sodammonium, and an alloy of antimony and sodium, $SbNa_3$, have similarly been produced.—On the analysis of minerals containing antimony, by M. Ad. Carnot.—On the microscopic structure of oolitic iron from Lorraine, by M. Bleicher. From the investigation it appears that the ferruginous oolites which have been studied consist of a central mineral or organic nucleus, single or multiple, surrounded by regular concentric layers of a substance rich in silica and organic matter.—On the vegetation of the vine, by MM. L. Roos and E. Thomas. Conclusions are given respecting the amounts of sugars present and the acidity of various parts of the vine plant at different stages of its growth.—Citric acid, by M. G. Massol. The heat of formation, in the solid state, of potassium and sodium citrates is greater than that of the corresponding carballylates. The augmentation is analogous to that observed when comparing malonic and succinic acids with tartronic, tartaric, and malic acids, and is to be attributed to the alcoholic hydroxyl group.—On some reactions of the isomeric amido-benzoic acids, by M. Oechsner de Coninck.—Calculation of the temperatures of ebullition of compounds derived from the paraffins by terminal substitution, by M. G. Hinrichs.—On the pyrogenous hydrocarbons formed in the compressed gas industry, by M. A. Brochet. The author has isolated and identified the following:—

$$CH_2 : CH \,.\, CH_2 \,.\, CH_3;$$
$$CH_2 : CH \,.\, CH_2 \;\; CH_2 \,.\, CH_3;$$
$$CH_3 \,.\, CH : CH \,.\, CH_2 \,.\, CH_3;$$
$$CH_2 : CH \,.\, CH_2 \,.\, CH_2 \,.\, CH_2 \,.\, CH_3;$$
$$CH_2 : CH \,.\, CH : CH_2;$$
$$CH_2 : CH \,.\, CH_2 \,.\, CH : CH_2.$$

—The specific gravity of silk, by M. Léo Vignon.—Glycolysis in the blood, by M. Maurice Arthus.—Are there inhibitory nerves?, by M. J. P. Morat.—On an anomaly in the great hypoglossal nerve, by M. Buffet-Delmas.—On the ovary and the egg of *Gobius minutus*, by M. Frédéric Guitel.—Note on the magnetic perturbations of March 11–13, 1892, by M. Th. Moureaux.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Note-book of Agricultural Facts and Figures, 4th edition: P. McConnell (Lockwood).—A Year-book of Science, 1891: edited by Prof. Bonney (Cassell).—Willing's British and Irish Press Guides, 1892 (Willing).—Sitzungsberichte der K.B. Gesellschaft der Wissenschaften, Math. Naturw. Classe, 1891 (Prag).—Abhandlungen der Mathematisch-Naturwissenschaftlichen Classe der K.B. Gesellschaft der Wissenschaften von den Jahren 1890–91. vii. Folge, 4 Band (Prag).—Health Springs of Germany and Austria, 2nd edition: F. O. Buckland (Allen).—The School Calendar, 1892 (Whittaker).—Silk Dyeing, Printing, and Finishing: G. H. Hurst (Bell).—Le Poil des Animaux et les Fourrures: Lacroix Danliard (Paris, Baillière).—Les Fleurs à Paris: P. L. de Vilmorin (Paris, Baillière).—Statistics of the Colony of Tasmania for the Year 1890 (Tasmania, Strutt).—Anatomie et Physiologie Comparées de la Pholade Dactyle: Dr. R. Dubois (Paris, Masson).

Pamphlets.—Neue Integrationsmethoden auf Grund der Potenzial—Logarithmal und Numeral-rechnung: Dr. J. Bergbohm (Stuttgart).—Neue Rechnungsmethoden der Höheren Mathematik, Dr. J. Bergbohm (Stuttgart). O Theorii Ploch: E. Weyr (V. Praze).—Jahresbericht der K.B. Gesellschaft der Wissenschaften für das Jahr 1891 (Prag).

Serials.—Beiträge zur Kenntniss der Orchideen von H. G. Reichenbach fil fortgesetzt durch F. Kränzlin; Dritter Band, Fünftes Heft (Leipzig, Brockhaus).—Proceedings of the American Philosophical Society, vol. xxix. No. 136 (Philadelphia).—Bulletin de L'Académie Imperiale des Sciences de St. Pétersbourg. Nouvelle Série, II. xxxiv. (St. Pétersbourg).—Bulletin of the New York Mathematical Society, vol. i. No. 6 (New York).

CONTENTS.

PAGE
The History of Determinants. By **P. A. M.** . . . 481
The Evolution of Man. By **A. M. M.** 482
Our Book Shelf:—
Bonavia: "Philosophical Notes on Botanical Subjects."—**W. B. H.** 483
Beddard: "The Zoological Record for 1890" . . . 483
Letters to the Editor:—
Sun Pillar. (*Illustrated.*)—**Annie Ley** 484
New Comet.—**W. F. Denning** 484
First Visible Colour of Incandescent Iron. (*Illustrated.*)—**Capt. A. Noble, F.R.S.** 484
Poincaré's "Thermodynamics."—**Prof. H. Poincaré** 485
Ornithology of the Sandwich Islands.—**Albert F. Calvert** 485
Superheated Steam.—**J. Macfarlane Gray** . . . 486
Phoronomy.—**A. B. Basset, F.R.S.; M. am Ende; G. C. R.** 486
The Tudor Specimen of Eozoon.—**J. W. Gregory** . 486
The Theory of Solutions.—**J. W. Rodger** . . . 487
The Limpet's Strength.—**J. Lawrence-Hamilton, M.R.C.S.** 487
Technical Education for Novelists.—**W.** 487
The Origin of the Year. I. (*Illustrated.*) By **J. Norman Lockyer, F.R.S.** 487
The Winter Storms of Northern India. (*Illustrated.*) By **Henry F. Blanford, F.R.S.** 490
The Magnetic Storm of February 13–14, 1892. (*With Chart*) 493
William Dittmar. By **A. C. B.** 493
Sereno Watson 494
Notes 494
Our Astronomical Column:—
Fuzziness of some Variable Stars 497
Astronomical Possibilities at Considerable Altitudes . 498
Increase of the Earth's Shadow during Lunar Eclipses 498
The New Star in Auriga 498
Aberration. (*Illustrated.*) By **Lord Rayleigh, Sec. R.S.** 499
Societies and Academies 502
Books, Pamphlets, and Serials Received 504

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Villing).
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PAGE
. 481
. 482
b-
. 483
. 483
. 484
. 484
us-
. 484
n-
. 485
F.
. 485
. 486
de;
. 486
y . 486
. 487
on,
. . 487
. 487
J.
. . 487
ed.)
490
With
. . 493
. . 493
. . 494
. . 494
. . 497
es . 498
pses 498
. 498
R.S. 499
. . 502
. . 504